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THE PAST AND FUTURE OF THE STUDY OF SOLUTIONS[1]

SOLUTIONS have been known since earliest times, and the problems which they represent have been studied by a long line of very able investigators. All of the early work on solutions has been inseparably linked with the study of chemical phenomena. Indeed, up to the year 1887 chemical views of solutions have predominated. So for example, in his lectures delivered at Yale College in 1837, Benjamin Silliman, Sr., considered solutions as chemical compounds; and in his memorable work on theoretical chemistry which appeared in 1863, Herman Kopp treated solutions as chemical compounds that exhibit variable proportions, which mode of treatment was retained by A. Horstmann when in 1883 he wrote the second volume of the new edition of Kopp's work, now known as Graham-Otto's "Lehrbuch der physikalischen und theoretischen Chemie." Ever since the days of Lavoisier, when the so-called law of definite proportions was first recognized, a distinction has been drawn between compounds which follow that law and combinations that do not. Chemical combinations which exhibit definite qualitative and quantitative composition that can not be varied gradually by small increments arbitrarily chosen were soon termed definite chemical compounds, whereas solutions, whose composition may be varied gradually, quite arbitrarily—at least

[1] Address of the vice-president and chairman of Section C—Chemistry—American Association for the Advancement of Science, Boston, 1909.

within certain limits—were regarded as indefinite chemical compounds, or compounds according to variable proportions. So Robert Bunsen used to teach that we may have compounds according to definite proportions and also compounds according to variable proportions, the latter compounds being the group known as solutions.

The careful quantitative investigation of solutions really dates from the time of Lavoisier, who, as is well known, introduced the balance into the chemical laboratory. Before this the observations made were generally only qualitative in character; at any rate they were often crude and faulty. The very fact that solutions were regarded as chemical compounds led to their study by much the same methods adopted for the investigation of definite chemical compounds, *i. e.*, chemical compounds in the narrower sense in which the term is at present commonly used. So the qualitative composition and the quantitative composition of solutions were carefully studied. The density, the color, the boiling point, the specific heat, the optical activity, the thermal accompaniment of the formation of solutions and of their reactions with other substances, as well as their other physical, chemical and physiological properties, were studied in much the same way that these various properties were determined for definite chemical compounds. And yet, the fact that the composition of solutions may be varied gradually and arbitrarily within certain limits and that this can not be done in the case of definite chemical compounds, has for nearly a whole century been considered to be the vital difference between a solution and a definite chemical compound, and this is quite proper.

To obtain a definite chemical compound in the pure state usually requires a considerable amount of work. The usual operations of purification as in vogue at present are crystallization, solution and precipitation, sublimation and distillation. By means of the so-called purification process a product is finally obtained whose composition does not change further, though the substance be subjected to further similar treatment. As F. Wald states it, a chemical compound is a phase whose composition remains constant though temperature, pressure and contact with other phases be varied within certain limits inside of which the substance in question is stable. In a sense then the so-called definite chemical compounds are really obtained in certain cases as the more resistant cleavage pieces resulting when the purification processes are applied. That the latter processes after all frequently represent rather violent treatment will probably not be gainsaid by any one.

The law of definite proportions was considered by Ostwald in his Faraday lecture, which in turn was discussed by others, among whom Benedicks voiced the sentiment that after all when closely scrutinized it becomes evident that there is an arbitrary element in judging as to when we really have a pure, definite compound before us, and that the matter of definite proportions is to some extent one of definition. As to the law of multiple proportions, this has been directly challenged by P. Duhem as a tenet that can neither be proved nor disproved, though I must frankly confess my inability to agree completely with him in his argument.

The year 1887 is noteworthy, for it brought both the van't Hoff theory of dilute solutions and the theory of electrolytic dissociation of Arrhenius. These theories really supplement each other, as is well known. They may well be called physical theories of solutions as distinct from the chemical views of solutions al-

ready mentioned. It is quite unnecessary to rehearse here the great activity that has resulted in the study of dilute solutions during the last two decades as a direct consequence of the theories of van't Hoff and Arrhenius. The pages of the history of chemistry that record this experimental work on dilute solutions will ever maintain their brilliant luster, for they reflect the enthusiastic efforts of scores of active young hands and minds that were urged on by a most inspiring leader, an able teacher and experimenter, and a most lovable man —Wilhelm Ostwald. Without him the theories of van't Hoff and Arrhenius would scarcely have gained a foothold.

But excellent as were many of the experimental acquisitions that were thus obtained as a result of these working hypotheses, time has shown that the latter have long since served their purpose, and that mere physical conceptions of solutions are untenable as an explanation of the phenomena actually observed. Furthermore, a theory which applies merely to very dilute solutions, and then only in an imperfect way, is quite untenable in the long run, even as a working hypothesis. It is not my purpose to enter upon a discussion of the numerous experimental researches which have made the theories of van't Hoff and Arrhenius untenable. These investigations have been published at various times during the last decade, and I have dwelt upon them in detail on previous occasions. It is quite safe to assume that they are sufficiently well known to all. Moreover, I frankly confess that I am glad to escape the task of recounting again the weaknesses of these views of solutions as exhibited by experimental facts, for in my younger days I was quite enthused with these hypotheses, and it was to me a great disappointment to find later that they were contradicted by so many experimental truths. It is rather my purpose to point out the direction in which experimental investigations made thus far have led us, and to attempt to indicate the line of attack which must be followed to insure success in the future, so far as this can at present be foreseen.

The data collected since 1887 in studying the various properties of solutions, though frequently gathered with the aid of the physical hypotheses already named, have nevertheless gradually and unerringly demonstrated that the chemical view of solutions is far nearer to the truth, than is the idea that a solution is a mere physical mixture. In this connection permit me to call attention to a few experimental illustrations.

When antimony trichloride and camphor are brought together the two solids liquefy each other, forming a thick syrupy solution, the proportions of the two ingredients of which may be varied within certain limits. Antimony trichloride and chloral hydrate similarly liquefy each other, though less readily. Again, camphor and chloral hydrate when in intimate contact with each other form a liquid. If now cane sugar or paraffine be treated with antimony trichloride or with camphor or chloral hydrate no change will be observed. The question arises, why do antimony trichloride and camphor liquefy each other and cane sugar and camphor not? It is perfectly clear that all that we can say is that this is because of the specific nature of the substances themselves. In other words, antimony trichloride and camphor liquefy each other and sugar and camphor do not for reasons that are similar to those which we give as to why charcoal will burn and platinum will not. We may say that the mutual attraction, *i. e.*, the affinity of antimony trichloride for camphor, is sufficient to overcome their cohesions, and so they unite and form the solution. Now as to whether the antimony trichloride dis-

solves the camphor or the camphor the antimony trichloride is clearly an idle question. We may regard either the one or the other as the solvent, for this is obviously a purely arbitrary matter. Let us now raise the following question: In the syrupy liquid that has been formed by the action of antimony trichloride and camphor on each other, how much of the camphor present is combined with the antimony trichloride that has been employed? The answer is perfectly obvious, for clearly all of the antimony trichloride is combined with all of the camphor in the syrupy liquid that has been formed. One might as well ask the question: When mercury and oxygen unite to form mercuric oxide, how much of the oxygen present is united with the mercury that the oxide contains? Clearly here too all of the oxygen is united with all of the mercury present. When the solution of antimony trichloride and camphor is heated, the vapor obtained contains both of the ingredients. Similarly when we heat mercuric oxide the vapor contains mercury and oxygen. We see thus that the cases are essentially similar in character, the only difference being that in the case of the solution in question we have a compound according to variable proportions, whereas in the mercuric oxide we have a compound according to definite proportions.

Now when ice acts on sodium chloride is not the case quite similar to that of camphor and antimony trichloride? Suppose we knew of no temperature above 0° C., would any one argue that the solid ice dissolved the solid salt in the process of forming the brine? Certainly not, we should say that the brine has been formed by the union of the ice with the salt. And here similarly the question as to how much of the salt in the brine is united with how much of the water in the latter is quite idle, for obviously all of the salt used has united with all of the ice. The case would clearly not be altered if we started with liquid water and solid salt and formed the brine by the interaction of the two substances. This view, that in a solution all of the substances present are united with one another just as all of the elements in a definite compound are combined with one another, is to my mind the only rational view we can take of the matter. It is not new; on the contrary, it is quite old. It has been held quite generally by scientists prior to 1887, when the physical theories came upon the stage and diverted attention into other channels, as already stated, with the result that the true nature of solutions has been thoroughly obscured. If now we dilute the brine with more water, does the water added combine further with the salt present? Most assuredly, for is not the vapor tension of a brine, however dilute, lower than that of pure water, and does not this show that the water in the brine experiences greater difficulty in evaporating because of the mutual attraction between the salt and the water? Were any of the latter uncombined with the salt of the brine, this uncombined water would show the same vapor tension as pure water; but a brine of the same vapor tension as pure water of the same temperature does not exist.

The phase rule of Willard Gibbs marks a great advance in the study of heterogeneous equilibrium. Through the practical work of Bancroft, Roozeboom and numerous other able chemists, the phase rule has borne rich fruits. In all of this work the composition of the phases that are in equilibrium with one another under given conditions of temperature and pressure was carefully determined. This work has revolutionized solubility determinations, placing them upon an accurate scientific footing. Nowadays when the solubility of a compound is to be thor-

oughly investigated nothing less than the complete equilibrium curves of the compounds in question will suffice; but once the work is carefully done, it is final for all time. This is not the place to dwell upon all the various questions that have been cleared up by the application of the phase rule. It should here be emphasized, however, that the latter deals with the equilibrium of the various phases whose qualitative and quantitative composition is of course ascertained. As to the inner structure of any one of the phases the phase rule is able to tell us nothing. Indeed, in the study of single-phase chemistry, the phase rule is no help whatever. We may consider the investigation of the constitution of definite chemical compounds a part of single-phase chemistry, and we may similarly consider the question as to the inner nature of a solution (*i. e.*, of a compound according to variable proportions) as a problem of single-phase chemistry. In the investigation of the constitution of single phases it is quite impossible to get along without hypotheses. While the phase rule does not involve even the atomic and molecular theories, these are at present indispensable tools in prying into the inner nature of any one phase. But in the study of solutions, interest centers not so much in the equilibrium between phases as in the inner structure of the latter themselves.

Our methods of ascertaining the structure of chemical compounds are quite numerous, but they readily fall into a few categories. So we argue as to the structure of a compound from its synthesis, from its analysis, from its behavior toward various other chemical agents, from alteration by the application of pressure, heat, electricity, light and kindred agencies, and also from its various physical and physiological properties. Thus, for example, it has always been considered as sound reasoning that because red precipitate can be formed from mercury and oxygen, these substances are in red precipitate, which conclusion is verified by the fact that the latter compound may be decomposed into oxygen and mercury. There has never been any objection to the argument that if one of the elements actually enters into a compound during the latter's formation, or can be obtained from the compound either in the free state or in combination with other elements, that element is actually in the compound. So since calcium carbonate may be made from calcium, carbon and oxygen, we argue that these elements and these only are contained in calcium carbonate. Again, when calcium carbonate is heated, calcium oxide and carbon dioxide, and these only, are obtained; and conversely calcium carbonate may be formed by the union of calcium oxide and carbon dioxide. These facts were duly expressed by the old dualistic formula for calcium carbonate $CaO.CO_2$ which consequently had much to commend it. Yet while we thus hold that the elements calcium, carbon and oxygen are in calcium carbonate, we do not argue that this compound contains calcium oxide and carbon dioxide, even though the last two substances will unite and thus form calcium carbonate, or though they may be obtained as decomposition products of the latter compound. We write our formula for calcium carbonate $CaCO_3$ because of the precipitation methods by which the compound may be prepared, and because of the formulæ that we assign to soluble carbonates on the basis of the products that they yield by electrolysis. We consequently hold that the carbon dioxide and lime that form when calcium carbonate is heated result from the rearrangement of the atoms and splitting of the compound on account

of the violence to which it has been subjected by heating it very highly. Similarly, while we recognize that carbon, hydrogen and oxygen are contained in cane sugar, we do not argue that the latter consists of water and carbon, though these products may among others be obtained by heating sugar. Likewise we are loath to conclude that proteins contain amino acids, simply because these result as cleavage products when the proteins are subjected to certain rather drastic treatment.

Turning now, for example, to a compound like blue vitriol whose composition we are wont to express by the formula $CuSO_4.5H_2O$, to indicate that it consists of copper sulphate plus water, we find that the water may be driven off by heat properly applied and that the dehydrated copper sulphate remains behind. On heating the copper sulphate further it is decomposed into copper oxide and sulphur trioxide. If it were intended to express these changes by means of a formula, surely the old dualistic formula $CuO.SO_3.5H_2O$ would best indicate what has been observed. But here again we have departed from the idea that copper sulphate contains copper oxide and sulphur trioxide because upon electrolysis of an aqueous solution of copper sulphate, metallic copper, sulphuric acid and oxygen are obtained; while upon adding zinc or iron to a copper sulphate solution metallic copper is thrown out, and the sulphate of the more basic metal results. So far as the water content of blue vitriol crystals is concerned, we only know its relative amount and that it can be driven off by heat, higher temperatures being required to secure complete dehydration, while relatively lower temperatures will suffice to remove a large portion of the water. As to how this so-called water of crystallization is held, whether it is united with the copper sulphate simply as water molecules adhering to the copper sulphate molecule, or whether, like the oxygen and hydrogen content of the cane-sugar molecules, the oxygen and hydrogen in blue vitriol are united with the sulphur and copper in some more complicated way, is an open question. So far as the facts known are concerned, they are expressed by the formula $CuSO_4.5H_2O$, just as at one time the formula $CaO.CO_2$ expressed what was known about calcium carbonate. To me it would seem very probable that the hydrogen and oxygen content in blue vitriol is not present as water molecules clinging to the copper sulphate molecule, but some subtle experimental method, as yet quite unknown, is required to elucidate this matter, and until such a method is found we shall continue to write our formula for blue vitriol as we are wont to do. It is perhaps well in this connection to allude to the well-known fact that many salts containing water of crystallization can not be dehydrated by heating them, for when this is attempted not only water, but other ingredients as well, are driven off, in other words further deep-seated decomposition occurs.

If crystals of blue vitriol be placed in water, a blue liquid is formed as a result of the action of the crystals and water on each other. This liquid we call a solution. The amount of water and blue vitriol used in its preparation may be varied arbitrarily within certain limits. For reasons already stated, this blue liquid contains no water that is not in combination with the salt present, and also no salt that is uncombined with the water. The fact is that this blue liquid is found to be perfectly homogeneous by all tests that we are able to apply. If we add more water to it, this additional water also combines with all of the salt present and the liquid is again

homogeneous; and this dilution may be carried on indefinitely. If, on the other hand, we permit the blue liquid to evaporate, we thus decompose it by abstracting water from it. We say that the solution is becoming more concentrated. This change is a perfectly reversible one, and like all chemical changes it follows the law of mass action. The abstraction of water from a solution of copper sulphate by means of heat is just as truly an act of decomposing that liquid as is the abstraction of carbon dioxide from limestone when the latter is heated.

Blue vitriol is formed by the addition of water to anhydrous copper sulphate. The compound thus produced is quite stable at room temperature. If now we add anhydrous copper sulphate to crystals of blue vitriol, the latter lose part of their water content, which is taken up by the anhydrous salt till equilibrium is established. If, on the other hand, we treat the blue vitriol crystals with water, it is clear that we can not thus dehydrate the crystals. On the contrary, this added water will, because of mass action, tend to increase the stability of the complex which we represent by the formula $CuSO_4.5H_2O$, and to this complex all of the additional water present in the solution adds itself. What then is the formula of the hydrate contained in an aqueous copper sulphate solution at known temperature? This question is really an idle one, for since all of the copper sulphate present is combined with all of the water of the solution, the composition of the hydrate is clearly expressed by $CuSO_4.xH_2O$, where x represents the number of water molecules which the entire solution contains per each copper sulphate molecule; and so x increases as we dilute the solution and diminishes as we concentrate it. But this must not be taken as meaning that all of the water in a copper sulphate solution is equally strongly bound to the salt molecules. Indeed, in the case under consideration it is extremely probable that at least five molecules of water are more strongly bound to each copper sulphate molecule in the solution, for as the salt separates out, these five molecules remain in combination as a part of the compound. But while in the solution the copper sulphate molecule plus five molecules of water may be present as a nucleus to which the additional water molecules are attached, the force of attraction with which the outlying water molecules are held by the nucleus shades off so gradually as the radius of the sphere of influence increases that there is at no point any very sharp demarcation, and so it would be folly to attempt to ascribe any definite formula whatever to the hydrate existing in the solution. Attempts to deduce the formulæ of hydrates in solutions from the boiling points or freezing points of the latter are very far from the mark, though to be sure boiling-point and freezing-point curves do frequently show maxima and minima which are doubtless due to changes of intensity with which the water and salt molecules are held together as their relative number is changed. Furthermore, it is very significant that such maxima and minima in the boiling-point and freezing-point curves are found in the case of those substances, which, when they crystallize from the solution, do so with one or more molecules of the solvent attached as so-called crystal water. It is well known that at higher temperatures salts separate from solutions with less crystal water than at lower temperatures. Indeed at high temperatures the anhydrous salt is frequently in equilibrium with the saturated solution. So while at ordinary temperatures copper sulphate forms crystals with five molecules of water, at lower

temperatures it may be obtained with seven molecules of crystal water. Now would it then be right to conclude from this that at room temperature the hydrate in the solution is $CuSO_4.5H_2O$ and at lower temperatures $CuSO_4.7H_2O$? Obviously not, but we may say that it is at least that indicated by the composition of the compound that separates. In the solution itself many additional water molecules are combined with the salt molecules, and the force of attraction gradually shades off as the radius of the sphere of attraction from the nucleus outward increases so that it is quite impossible to ascribe any definite formula to the hydrate in the solution. (I should like to add parenthetically here that the recent attempts made to draw conclusions as to how many water molecules are attached to a portion of certain salts, from observations of changes of concentration that occur at the electrodes during electrolysis, are also based upon misapprehensions, but these details can not be taken up here.) It is, moreover, well known that when any physical property of a solution is studied at different temperatures the curve representing the alteration of that property with change of temperature does not show sharp points of inflection, indicating that whatever the internal alterations may be within the solution, they occur gradually rather than suddenly.

In the study of the various physical properties of solutions with changing temperature and changing concentration, it has been absolutely demonstrated that different solutions behave quite differently, and that solutions of compounds that are chemically analogous show an analogous, but by no means an identical, behavior. It is consequently quite impossible to write an equation that will hold for the various known solutions—not even approximately. Attempts to formulate an equation for a so-called perfect or ideal solution are about as successful as an attempt to write an equation for an ideal or perfect chemical compound would be. In short, such equations are necessarily based upon postulates that are not in accord with experimental facts, and consequently the equations themselves can not and do not agree with what is actually observed. The attempts to parallel solutions with gases in a quantitative way would naturally suggest that there might be an equation for an ideal or perfect solution just as we are wont to write an equation for a so-called ideal gas, but the suggestion is quite misleading, just as all of the efforts at a quantitative study of solutions based upon gas analogies have proved futile. This is true not only of solutions of moderate concentration, but of dilute solutions as well, as a careful unbiased scrutiny of the numerous experimental data that have been collected shows.

The act of solution is accompanied by all of the phenomena that are observed in the case of changes that are regarded as chemical by common consent, and this shows that solutions are chemical in character. We commonly say that whenever substances combine chemically with each other, the new substance formed has properties that are quite different from those possessed by the original substances. While this is true, it is also the case that some of the properties are not changed at all, while others are but slightly modified, and still others are very greatly altered indeed. So, for instance, the weight remains unchanged during chemical action; the specific heat is frequently altered but little, whereas the color, volume and other properties may be very greatly affected. In general, we may say that *when an element or compound enters into combination with other elements or compounds, each of the ingredi-*

ents of the new substance formed tends to retain its original characteristics as far as the new conditions to which it has been subjected permit. In reality every chemist is well aware of this, though as far as I know the idea has never before been stated in so many words. The degree to which an element loses its original properties on entering into combination with other elements depends very largely upon whether the chemical change involved is a drastic or a mild one, which in turn is principally determined by the energy accompaniments of the reaction. In the study of solutions, which in general represent rather compounds formed by relatively mild changes as compared with many of the stereotyped chemical reactions, the thought just expressed is particularly helpful. So, for instance, sodium has a great affinity for the elements of water, upon which the solubility of sodium compounds in water largely depends. On the other hand, sodium is inert toward hydrocarbons, which fact is at the basis of the insolubility of sodium salts in hydrocarbons. An element with pronounced chemical characteristics like sodium, for example, will retain to a high degree its chemical predilections even after it has entered into combination with other elements. Thus if we take sodium oleate, in which the metal is combined with the large fatty oleic radical, we nevertheless find that this soap dissolves in water. Here again the great affinity of sodium for water manifests itself, and though the metal is chained to the fatty radical which of itself exhibits no inclination to unite with water, yet this radical is dragged along into solution as it were by the great chemical attraction which sodium still has for water. But the combination which water and sodium oleate form is after all but a loose one, as one would naturally expect from what has been stated. The fact that a solution of sodium oleate boils but slightly higher than pure water shows that there is but little affinity between water and the soap. Again, the insolubility of sodium oleate in hydrocarbons shows that the oleic radical, though it is known to have affinity for hydrocarbons and fats, is yet unable to drag the sodium with it into solution. On the other hand, however, the affinity of the oleic radical for fatty substances does manifest itself when a strong aqueous soap solution is brought into contact with greasy matter on clothes, etc., for by virtue of this affinity the grease is loosened from the fabrics, and though not dissolved, it is nevertheless emulsified so that it can be removed mechanically with the soap solution. Numerous other examples illustrating the principles stated might here be mentioned. I am at present engaged in the work of collecting these. Before the advent of the physical theories of solutions considerable work was done in ascertaining the chemical relationships that must exist between solvent and solute in order that solution may take place; but during the last two decades this work has been practically discontinued, which is particularly unfortunate. It clearly indicates, however, how our so-called modern conceptions of solutions, which have been pressed upon the scientific public by a species of propagandism that is, and it is to be hoped will remain, quite unrivaled in the history of chemistry, have really stood in the way of progress.

In some quarters the idea is still prevalent that electrolytes are essentially different from non-electrolytes in their chemical behavior. This is thoroughly fallacious, for *all chemical changes that occur in electrolytes can now be reproduced as to type and as to rapidity in the best of insulators. An electrolytic solution behaves*

like any other solution, except that it has the property of conducting electricity with concomitant chemical decomposition. There is no way known at present by which any one can foretell whether a given solution will conduct the current or not. The only way to find out is by actual trial with the electric current itself. There is also a misapprehension that only electrolytes will cause the coagulation of colloids. Such coagulation can be quite as well accomplished by non-electrolytes, so that here too there is no essential difference between electrolytes and non-electrolytes. Upon what electrolytic conduction really depends we are still quite ignorant, just as we do not know why a bar of silver conducts and a stick of sulphur insulates. But upon this matter I have already expressed myself more fully on other occasions.

Again it is necessary to call attention to the fact that there is really no essential difference between colloidal solutions and solutions of crystalline substances. I do not refer to those so-called colloidal solutions which from the very mode of their preparation must be regarded as suspensions, which view has also been confirmed by the use of the ultramicroscope. *We are now able to separate crystalline bodies from each other by dialysis, also crystalline bodies from those that have never been obtained in the crystalline state by having the latter pass through the septum and the crystalloids remain behind; and indeed, even two colloids may be separated from each other by dialysis,* as I have demonstrated experimentally in the course of my researches on osmosis. The matter depends entirely upon the nature of the solutions and the chemical nature of the septum, and from a knowledge of these, what will happen may be foretold.

Water is a great solvent, and because of its abundance and importance to all life on the globe aqueous solutions will ever be studied with the greatest interest. But in obtaining a correct conception of the nature of solutions, aqueous solutions obviously can have no stronger vote than solutions in less abundant and far less readily procurable liquids. Water has a high cohesion, as is shown by its high surface tension and high latent heat of vaporization. The hydroxyl group which is characteristic of the water molecule certainly exhibits great tendency to cling to other hydroxyl groups. So, for instance, though hydrocarbons are not soluble in water, they become soluble when one of their hydrogen atoms is replaced by hydroxyl, provided that the number of carbon atoms in the compound is small. However, when more than one hydroxyl group is in an organic compound, the latter may have even a relatively high carbon content and yet be soluble in water. A study of organic hydroxyl derivatives shows that compounds consisting of carbon, hydrogen and oxygen, and containing one or more hydroxyl groups for every carbon atom present, are soluble in water, though, to be sure, even considerably less than one hydroxyl group per each atom of carbon in the molecule is frequently sufficient to cause solubility. On the other hand, the multiplication of hydroxyl groups in such compounds tends to diminish their solubility in hydrocarbons. From this and similar illustrations that might readily be given it is clear that a study of the solubility of a compound in different solvents may well serve as a means to investigate the nature of that compound.

It need not be feared that by accepting the chemical view of solutions we should lose the advantage of the molecular weight determinations by the boiling-point and freezing-point methods. These methods would serve us as well as ever. But we should not argue that common salt is dis-

sociated in water because a gram molecule of it added to a liter of water produces a solution that has a higher boiling point than the solution obtained by adding a gram molecule of sugar to a liter of water. We should rather hold that the higher boiling point of the former solution is due to the greater affinity between salt and water as compared with that between sugar and water.

The study of solutions then was begun with the chemical conception of solutions, and upon this conception many relationships have been worked out during the first eighty-seven years of the nineteenth century. The older chemists clearly recognized that whether solution will take place or not in a given case is first of all determined by the chemical nature of the substances brought into contact with each other. They saw that the temperature factor was next in importance, and that pressure was of vital consequence when a gas was under consideration, but of slight importance in the case of solids and liquids. When the conception that solutions are mere physical mixtures came to the foreground, through the introduction of gas analogies and the intense propagandism of the dilute school, the fact that the act of solution is really chemical in character was lost sight of by many able, enthusiastic young investigators. In the ardor of their quest they were misled, and unwittingly they naturally misled others. It is really pitiable to see how our physiologists, having thus taken up these misconceptions of the nature of solutions, are still wasting precious time in endeavoring to work out the complicated and very important processes that occur in living plants and animals. In these problems, which are in reality perhaps the very greatest that confront us at the present day, theories of solutions based on gas analogies are of no avail. They are thoroughly misleading and worse than worthless here.

The clear recognition that solutions are really chemical in character and that there is no wide gulf that separates the act of solution from other chemical phenomena, will do much toward furthering the future study of the subject. I do not claim to have prophetic ability, but nevertheless I venture to express it as my conviction, based upon years of experimental study of the chemical, physical and physiological properties of a long list of both aqueous and non-aqueous solutions, that the act of solution is chemical, that solutions are chemical combinations, and that we can only make real progress toward a better understanding of the various solutions by recognizing this as the basis of all of our future work. The efforts to gain a better insight into the different solutions that confront us must be chiefly experimental, rather than mathematical; for in the study of solutions, just as in the study of chemical compounds in the narrower sense of the word, we are continually confronted with discontinuities. Now discontinuous functions can not be handled mathematically at present, not even by the greatest of our mathematicians, for though work of this kind has been begun, it is still in a very rudimentary stage. It is highly probable too that the renewed study of solutions from the chemical point of view will greatly aid us in getting a broader and more correct conception of the nature of chemical action itself. Certainly in living beings we have numerous, fundamental and deep-seated chemical changes going on continually with apparently the greatest ease at ordinary temperatures and pressures, and it is tantalizing that we are unable to comprehend how this is all brought about. In the unraveling of the

questions that here confront us a clear recognition that solutions are chemical in nature will be of greatest service.

Louis Kahlenberg

ON THE NATURE OF RESPONSE TO CHEMICAL STIMULATION[1]

In its last analysis we may readily enough suppose that the response of organisms to any stimulus is indirectly, at least, a result of chemical stimulation. That is to say, we may suppose that any change of environmental or internal conditions, whether it be of a chemical nature or of what is ordinarily called a strictly physical nature, awakens response by reason of chemical changes which are induced by its action, and these chemical changes are themselves the starting point for the chain of reactions which eventually evince themselves as the response.

A factor like increase of temperature very likely depends for its effect considerably, if not very largely, upon the chemical readjustments which it causes within the protoplasm. We have of course in the first place what might be called the primary or unmodified effect of increased temperature—the general acceleration of chemical processes which under such conditions is axiomatic in both inorganic and organic reactions and which does not necessarily imply any change in the chemical constitution of the protoplasm. But we should not assume too readily that the case is as simple as this, for organisms do not respond in the manner in which they would were their protoplasm a stable compound. In short, we are justified in supposing that certain changes of a more or less profound nature, due to altered chemical constitution, are the net result of rise in temperature. For instance, a change of temperature will increase the intracellular activity of the protoplasm and may readily disturb the balance of the metabolic processes so that the production of a larger amount of excreted waste products will further accentuate or perhaps even modify the response by reason of a purely chemical stimulation caused by these very waste substances. Again, it is well known that one of the critical points of protoplasm as regards temperature—the coagulation point—depends upon the amount of water held by the protoplasm, including without doubt chemical as well as physical constitution of water. The less water, the higher the coagulation point, or in other words, the less water the less readily the final chemical reaction of protoplasm to heat takes place. The longer the organism is subjected to new conditions of temperature the more permanent the changes become, as is shown by the phenomena of acclimatization; and the more gradual these changes are, the less likely are they to result in the destruction of the plant.

In the response of protoplasm to light we have another instance where an external physical factor affects the chemical structure within the organism and thereby sets up reactions which are traceable to chemical stimuli. Without referring to the action of the red-orange rays in photosynthesis, I may call your attention in this regard to the action of light as a whole as a formative stimulus in tissue differentiation. In the absence of light, as is now well known, the production of the more elaborate prosenchymatic tissues is, to a large extent, if not wholly, inhibited. Now we can not suppose that light rays alone are directly responsible for, let us say, the lignification of the mechanical tissue in a stem, but their action is to cause certain chemical changes which constitute the stimulus which enables the living tissue to

[1] Address of the vice-president and chairman of Section G—Botany. American Association for the Advancement of Science, Boston, 1909.

build up this particular form of cell wall. It is interesting too to note in this connection that certain poisons of a purely chemical nature have the same effect in retarding tissue differentiation as does the absence of light. This would seem to indicate that this particular phase, at least, of the response was due to a form of auto-intoxication of the normally illuminated tissues when grown in the absence of light.

It is, however, not necessary to dwell further on this aspect of the question. Few, if any, physiologists would to-day be inclined to deny the ultimate chemical nature of the response of protoplasm to any form of stimulus. It is the purpose here to limit the examination of chemical irritation more especially to actual concrete chemical substances brought into relation with living protoplasm, and to inquire somewhat more particularly into their mode of action and the nature of the changes which they induce. The importance and fundamental nature of these reactions can not be doubted.

For this purpose we may include in the list all those substances which it may reasonably be believed induce, by their chemical action, constitutional changes in protoplasm. These substances may be mineral salts, organic compounds of great diversity of structure, including anesthetics which have been perhaps wrongly placed in a special class, and even gases of a simple constitution. They may be crystalloidal, electrolytes or non-electrolytes, or perhaps even colloidal.

As a starting point it is necessary to admit that there are certain chemical elements which must be supplied to the plant for what is considered its normal development. Ordinarily these elements are supplied to the autotrophic plant in the form of oxygen, carbon dioxide, water and solutions of certain mineral salts, with the substitution in the case of heterotrophic forms of some suitable organic compound of carbon. For all of these necessary simple substances there is supposedly an optimum tonic point of concentration, though experience shows that it may vary somewhat, and the same is true of the more complex organic food supplied to the plant devoid of chlorophyll.

Moreover, not only must these substances be presented in an acceptable form and in the proper concentration, but there must also be a proper physiological balance in the mixture of the raw foodstuffs. The relation of the plant to the so-called normal food supply is not the question which it is here specifically our purpose to discuss, and we may assume that the plant is supplied, under the most favorable conditions, with sources of raw food material and is under the influence of favorable external conditions.

However, there are some points in regard to the normal food supply which have a direct bearing upon the question of chemical stimulation, as defined even in its restricted sense, which should be referred to before passing on to the main subject. In the case of some of the necessary food materials the concentration may vary within relatively wide limits before the effects of a lack or excess of these substances are observable. In such cases the increase necessary to produce a reaction may readily be so great as to involve a material increment in the isotonic coefficient of the solution and thus confuse any result produced by any direct chemical stimulus with those initiated by the change in osmotic pressure. Potassium salts, for example, will fail to elicit any response in the growth of fungi until the concentration is so increased as to raise the osmotic pressure by several atmospheres. It is known, however, that some of the necessary salts which are required by the plant in relatively

small quantities may, if the concentration be raised above the normal point, cause a secondary stimulation of growth and eventually, if the increase be continued, become inhibitory after the manner of poisons. Iron salts accelerate the growth of certain fungi far above the normal, when present in even slight excess, although the increase in concentration is nowhere nearly sufficient to raise measurably the osmotic coefficient of the solution. It has likewise been shown that under certain conditions calcium and magnesium salts seem to stimulate growth in a manner which may be considered strictly chemical, although with some plants the added concentration makes necessary a consideration of possible osmotic changes. It should also be said that in the case of the relation of calcium and magnesium the question of physiological balance between the two appears to be especially important, though this of course would not apply to fungi where magnesium alone is required. The question of the rôle of the elements which are needed in only very small quantities, especially in the case of iron, is a highly interesting one and it is strongly suggested that they are in their normal relation to the protoplasm of the nature of chemical stimulants rather than of necessary food elements. Calcium would not indeed come under this head if, as some believe, certain calceo-proteids are essential constituents of the living substance, but for iron and to a lesser extent magnesium and perhaps even potassium a purely chemical relation is highly likely. Iron salts at least may simulate the action of a catalytic agent, a point of view which will be more fully explained later.

In any event, in speaking of necessary raw food material, the question must be regarded as a purely relative one, and one should not cling too closely to the conventional idea of what a plant must be provided with. A multitude of special cases show that the relation of protoplasm to the so-called necessary elements may be very different in different cases. Anaerobic bacteria, for instance, are exceedingly sensitive to free oxygen, the presence of infinitesimal quantities of which in the case of certain Spirilla acts as a stimulus to induce a vigorous negative tactic response. Again, among the nitrifying bacteria forms are known where the presence of sugar, usually so acceptable to non-chlorophyllous plants, acts unfavorably. Instances of this sort might be multiplied, but it is the purpose at this time simply to call attention to the fact that chemical stimulation and eventually even toxic action may result from the presence of substances ordinarily regarded as necessary to sustain life.

It is indeed the case, then, that any substance whose presence may influence the behavior of a plant either normally or abnormally is of the nature of a chemical stimulus and therefore belongs to the topic under discussion. Since, however, our knowledge along these lines is very scanty and since we can from ocular evidence recognize what may be fairly termed a normal growth in a plant, I prefer to assume for the time, as has already been stated, that a plant furnished with the necessary food materials to produce its typical morphological development and with these substances in optimum concentration, is in a state of equilibrium as far as chemical stimulation in its restricted sense is concerned.

In this connection attention may be called to what appears to be an error in the point of departure of some investigators who have endeavored to determine the relative stimulatory value of certain substances, whether these be necessary or not to the plant. The mistake comes in the reference to distilled water as the medium

in which control cultures are grown, the variant being the same distilled water plus the substance under investigation. It is obvious that metabolic processes and consequently growth can take place only in such plants or plant parts in which elaborated food material is stored. It is equally obvious that the osmotic relations must be disturbed. Besides the lack of chemical balance there is also a lack of physiological balance. Plants under experimentation to determine the effect of chemical stimulation should be referred for comparison to those grown under conditions which are as nearly as may be the ones which can be recognized as producing opportunity for what experience shows is the natural morphological development of the organism. The physiologist should no more neglect the morphological aspects of his investigations than should the morphologist the physiological.

In its restricted sense, then, chemical stimulation may be said to deal with the effects of chemical agents which are not only not necessary, but which may be positively deleterious to the organism—poisons in short. It has been established that many, if not all, classes of substances which exert a toxic action on protoplasm will become stimulatory if presented to the cells in sufficiently small doses. Somewhere between an infinitesimally weak solution which produces no reaction, to the toxic dose which kills there is a stimulative optimum which gives the maximum of reaction. Experience shows that this is true of widely different substances—a poisonous gas like carbon monoxide, a poisonous metallic salt like copper sulphate, a simple organic compound like chloroform or a more complex one like an alkaloid, all come under this head. The question which concerns this paper is not the possible ultimate lethal effect of these poisons, but how far they may serve to excite the protoplasm to extraordinary activity. The amount required to effect the latter result will naturally vary with the substance, certain mild poisons possibly never affecting the plant beyond the stage of stimulating growth, no matter how high a concentration was employed.

From the work of Raulin and others, it is known that metallic salts in themselves toxic to protoplasm will, if presented to it in minimal doses, accelerate vegetative processes in a variety of plant forms. Certain fungi may be made to develop their vegetative hyphæ much more luxuriantly by the addition to their nutrient substratum of quantities as small as .0005 normal of zinc sulphate, and the increase of dry weight of cell substance produced may exceed by 200 per cent. or more that which is formed by similar cultures without stimulation. Nor is this limited to salts of the heavy metals, nor indeed to inorganic substances, for organic substances such as glucosides and alkaloids, or even simpler ones like chloroform, produce a similar if not so marked result.

In the concentration necessary to produce the characteristic reactions there is great diversity. As would be expected, not only are different substances very unequally stimulatory or toxic, but also the same substance varies greatly in the amount required to stimulate different organisms. Copper sulphate, one of the most violent of poisons to plant protoplasm, does not inhibit the growth of *Penicillium* until a concentration of nine per cent. is reached, yet the effect of the same salt is so enormously poisonous to many algæ that an infinitesimally weak solution will speedily cause death. What is true of the toxic point is true also of the stimulatory optimum. In the attempt to explain such disparities stress has been laid by some on

the probable impermeability of the cell membranes to this highly toxic salt in the case of the resistant forms. I am inclined to believe myself, however, that it is probably not so much due to such causes as to specific differences in the constitution of the protoplasm itself, which renders the usually poisonous substances relatively inoperative. There hardly seems enough evidence to support the idea of any very highly specialized qualitative selective power on the part of the cell membranes in the matter of dissociable and diffusible salts. On the other hand, there are many reasons for looking upon protoplasm not as a uniform substance, but as differing considerably in different plants. The fact that some plants can not thrive except in the complete absence of oxygen is enough to illustrate this point. The condition of the stimulated plant may itself cause a variation in the optimum concentration of the stimulant, as is shown by the effect of rise in temperature on the lowering of the toxic or stimulatory dose. It is not only among lowly organized plants like fungi that stimulation follows such conditions, but among the higher vascular plants as well. We can not suppose that these stimulants react directly upon the protoplasm or themselves supply the energy necessary for the changes which they induce with a possible reservation in the cases of those salts whose valency may be subject to change. In the first place, they fall in very different groups of toxic substances, if we take Loew's well-known classification, and yet there is a great similarity in the reaction produced. Therefore it is reasonable, for the time being, to disregard to a considerable extent the question of the chemical nature of the stimulating substance as far as its effects in accelerating the life processes of the organism are concerned. This does not mean, however, that the ultimate effect on the manner in which these poisonous substances may, in strong solutions, kill the protoplasm is not related to the chemical nature of the toxic agent. Not only are the stimulants not the sources of energy for the changes involved, but also they can not, in most cases at least, be regarded in themselves as catalyzers, no matter how greatly the end result of their action might suggest their being of such a nature. If, therefore, we are to find any satisfactory clue to the answer to the question of the influence of these minute doses, we must look rather towards the indirect effect they may exert and endeavor to discover if they may not encourage the formation by the protoplasm itself of substances which do act in a catalytic fashion. It seems clear, then, that the poisonous action of a given substance may be, and probably commonly is, very different from the stimulating effect of small doses of the same substance.

Whether it is safe to say that all substances which are toxic must of necessity act as stimulants if presented in sufficiently dilute form is a question. It is conceivable that some might produce no reaction unless present in a lethal dose, but it seems probable that most substances will show a stimulating reaction at the proper dilution. In this connection it is well to remember that we should not confuse the necessarily more complicated reaction of higher animal forms, whose balance of function is so delicate and whose tissue structures are so very diverse, with the more fundamental and presumably simpler and more direct reactions of the less interdependent cell aggregations such as are found in plants. It is reasonable to suppose, however, that as far as the cells themselves are concerned the underlying principles are much the same in all organisms.

Upon inquiring more closely into the effects of stimulants, we find that while a

great deal is unknown there are a number of important facts concerning which there is positive information and which throw considerable light on what is really taking place under such conditions. It is best, before taking up the physiological reactions, to consider the morphological changes which ensue, which, if we wish to employ modern terminology, we may term "chemomorphosis." The information regarding the lower forms—particularly the fungi —is the fullest and will be considered first.

The primal fact of the increase on dry weight has already been spoken of and is the simplest of all the reactions to demonstrate. By the easy process of the desiccation and weighing of a series of cultures the stimulation curve of the whole range of possible concentrations from minimum to maximum may be determined. Although it must be said that to obtain definite results means which might seem to some to be exaggeratedly careful must be taken to ensure the purity of all substances entering into the culture medium. Not only is the quantity of mycelium formed greater, but also the form and appearance are very different. Fungi commonly cease to form conidia under stimulation, the mycelial felts are buckled and knotted instead of being flat and even and their consistency is different, being tough and leathery instead of somewhat tenuous in texture as in the normal growth. In short they present every appearance of more luxuriant vegetative activity. The cell forms are often different, especially among bacteria, where the so-called involution forms arise apparently from chemical stimulation. Among many of the fungi, at least, such conditions are tantamount to a state of hyperplasia, if we may use the term in speaking of such lowly organized forms. Among the higher plants there may be simply an increased rate of growth and an ultimately greater stature, or, in other cases, as in the local application of metallic salts in initiating local intumescences or in hastening and increasing the formation of wound tissue, actual hypertrophies may be induced. In the stimulation afforded by parasitic fungi or by gall insects the great expression of abnormal growth is to be seen amounting often to relatively large outgrowths of tissue. The reaction in these various cases would seem to differ rather in degree than in kind, and it is perhaps a question not in this case of a mere hastening of growth, but of the excitation being sufficiently violent to destroy the equilibrium of growth which exists among the cells.

In no such case, however, have we any evidence that the variations in form so induced are inheritable. It is only when the germ cells at or near the time of their formation are directly stimulated that we get any changes in the offspring which are passed on to the succeeding generations. Sometime, it may be, means will be found by which an excitation of the sporophyte can be made in some way to influence the gametophytic cells and thus induce permanent variations through influences brought to bear indirectly upon the gametophyte, though it is not to be supposed even in that case that the particular response induced in the original sporophyte will be repeated in its offspring.

It is evident, from the effect of parasitic fungi upon their hosts, that not only does the stimulus of the parasitism of a specific fungus produce more or less specific results, but also that the condition of the parasitized cells themselves influence the result. The more primitive or meristematic are the cells the greater the resultant effect in the way of a distortion, for, as is well known, the greatest hypertrophies take place when the infection is in the

growing points of the shoots and becomes less and less when the more stable and permanent tissues like leaf parenchyma are attacked. The same fungus which causes real hyperplasia in young tissue produces but a hypertrophic enlargement in the adult parenchyma.

Such being the case, one might be warranted in reasoning by analogy that the still more plastic cells of the gametophyte would be even more profoundly influenced by stimulation and such indeed appears from MacDougal's experiments to be the case. It is also not unreasonable to suppose that the inciting cause of the healing of wounds, of stimulus to growth after injury, and even of regeneration phenomena themselves, harks back to a question of chemical stimulation. In the more massive tissues, at any rate, wounding results in the exposure of interior cells directly to the action of the oxygen of the air, and is accompanied by increased metabolic activity. The more rapid growth of injured parts, the awakening of dormant buds, may well be influenced or initiated, though probably not eventually controlled, by chemical stimulation arising from this or similar causes.

It is not to be supposed in any case that the chemical substances in question themselves constitute—by any direct union with the protoplasm—the modifications which ensue. It is only possible here to touch thus briefly upon the morphological responses induced by chemical stimulation, for the field is an enormous one. In some of its aspects, the study of the immediate effect of environment upon the external and internal form of a plant comes under this head. There is without question a large and inviting field for investigations into the nature of the changes in structure which are correlated with chemical stimulation.

It is necessary also to pass over without further comment the directive effects of chemical stimulation upon growth and movement, concerning which there are many investigations as to the expression of the reaction, but very little information as to the intimate causes of it.

After this brief consideration of the changes in the actual amount of elaborated substance, of stature and of structure which commonly attend chemical stimulus and which are the outward signs of its workings, we may next turn to the more fundamental question of what we know of the influence of this excitation on the physiological activities of the plant.

One fact which is clearly marked in the case of certain fungi that have been investigated is that the protoplasm, when stimulated, works more economically in respect to the carbohydrate food material supplied than when unstimulated. The latter produces a larger crop, as estimated from the dry weight, from a given amount of sugar than the normal culture does: there is less waste. Were the metabolic activity of protoplasm to be interpreted simply in terms of economy of action, one might be tempted to speak of such a condition as more nearly approximating a perfect or so-called normal; but when we reflect that we know so little of the chemical action and interaction of living protoplasm, it would be unwarranted to assume that mere economy of consumption of one form of food material would tell the whole story. The plant is attuned to an average condition and its attunement to that condition constitutes the nearest approach to what we may call normal. The increased availability of the sugar under chemical stimulation may be regarded as an untoward, fortuitous condition which, while it may be optimal for the processes involved in building up vegetative hyphæ, is not optimal for the development of the

plant as a whole, as is evidenced by the suspension of spore formation. This increased availability of the carbohydrate food is, then, distinctly unusual as far as the ordinary life processes of the fungus are concerned. The cessation of conidial spore formation which characterizes even slight stimulation is a morphological abnormality in the usual life cycle of the mostly asexual hyphomycetous fungi, and while it might be argued that spore formation is an evidence in itself of at least the initiation of unfavorable conditions, such considerations hardly apply here. It would be true only in a very limited degree, for the stimulus to spore formation need not necessarily be inimical in any large sense of the word. Whether it is the more economical working of the protoplasm which inhibits the formation of conidia or whether the absence of the latter results in less waste of energy in metabolism is perhaps a question, though probably most would agree that the spore-forming process is one that demands a greater expenditure of energy than the mere vegetative growth of the hyphæ.

From what we know of the effect of chemical stimulants upon the eggs of organisms, it would look as though the processes set up by such excitation are more critical for the sexual cells than for those of what may be regarded as the sporophyte. It would be exceedingly valuable to discover more about the relation of chemical stimulation to the production of gametes or their equivalents, and here we have another attractive field that has not been largely cultivated. It may be said in passing that as far as these non-sexual hyphomycetous forms are concerned there is not much evidence to show that such chemical stimulation as has been tried is sufficient to restore the ability, in many cases long lost, to produce sexual fruit.

It has been stated that there is less waste as well as a greater economy in manufacture of dry substance. One would naturally suppose that the two go hand in hand, but it is well to specify more definitely in what this smaller waste consists. One of the characteristic products, though not indeed necessarily an end product of katabolic activity in the plant cell, is oxalic acid, particularly in the case of these same fungi which we have been considering, where it is freely excreted into the substratum. Now the amount of this may be determined with relative ease, and it has been shown that with a stimulated crop there is a marked decrease in the ratio of the oxalic acid formed to the amount of dry substance produced in a given time. Together with this the carbon dioxide production does not appear to much more than parallel the increase in the weight; or in other words, the formation of this gas is approximately normal. This being the case, it at once becomes evident that the carbohydrate represented by the difference of oxalic-acid production in the normal and in the stimulated plants is at the disposal of the organism in the constructive processes. As for the higher plants, it has been shown that an increase in carbon-dioxide production takes place under stimulation, but these results are hardly complete, having been made without reference to net gain in substance. This matter should be further investigated, since it appears that the formation of wound tissue, when subjected to stimulation, is accompanied on the average by a greatly lessened carbon-dioxide production as compared with unstimulated growth; and this, too, in spite of the fact that there is ocular evidence that greater cell activity results under conditions of stimulation.

A highly interesting and instructive light on this question is thrown by the be-

havior of *Sterigmatocystis nigra* in relation to its assimilation of nitrogenous material. This fungus has the power, to a limited extent at least, of assimilating free nitrogen from the air. Stimulation appears to diminish this ability and to cause the fungus to rely more largely upon the nitrate fed to it: or at any rate the organism does not excrete into the liquid substratum as large an amount of waste nitrogenous products as does the normal. Furthermore, the nitrogenous content of the dry substance of the plant is not affected one way or the other. In regard to the nitrogen supplied in combined form, there is less thrift in the stimulated than in the normal growth, but, on the other hand, the total amount of nitrogen involved, including that excreted as waste into the substratum, is less in the former than in the latter case. This whole question is, of course, a hugely complicated one and in the light of our relatively slight knowledge of nitrogen metabolism one which should be approached with caution. But it is evident that the problem is of great importance.

In this connection it is apropos to quote from practically the only investigation we have which touches on this point.

> To explain the reason for the activity of the organism along these lines there are these suggestions: one that the fixation of free nitrogen and its excretion in combined form may be a function connected with fructification, since stimulated felts do not produce spores; another . . . is that the stimulated crop, driven to its most rapid metabolic activity by the stimulant, is forced to consume its carbohydrate more economically and therefore finds less energy to use in effecting the combination of the relatively inert and difficultly combinable nitrogen, and so must use the more readily assimilable compound nitrogen; or again it may be that since by the presence of the stimulant the fungus can consume carbohydrate more thoroughly and with less waste, therefore it finds in what would be a normal amount under ordinary circumstances a more than necessary amount under the favoring influence of the stimulant, which, of course, would be then potentially a too great supply, and the result would be over feeding in this direction and therefore there would be a tendency to lessened activity in expending energy for nitrogen combination. This last hypothesis is in accord with conclusions that have been reached on the activity of the root tubercle bacteria in fixing nitrogen when well supplied with nitrogen compounds, but not in accord with the results of those who find that the fixation of nitrogen is directly proportional to the amount of sugar at hand.

After consideration of the whole matter, one is inclined to the opinion that, after all, since less nitrogen passes through the fungus for the amount of dry substance formed, there is economy in nitrogen as well as in carbohydrate metabolism in a stimulated growth. And taking it all in all, there seems to be sufficient evidence for maintaining that under chemical excitation of optimum intensity the waste involved in mere cell formation, at least, is not so great in stimulated as in unstimulated protoplasm.

In this connection there arises at once another question of great importance, namely, what influence stimulation has on enzymatic activity. While the data on this point are still incomplete, it is permissible here to make reference to certain results not yet completed which throw some light upon this phase of the matter. Here again *Sterigmatocystis nigra* is valuable for experimentation. In common with many of its kind, this fungus can live on a great variety of substrata, its ability to do so being due in large measure to its versatility in the excreting of an enzyme appropriate to the particular compound on which it is growing. Thus it will produce maltase when grown on maltose, sucrase when grown on saccharose, inulase on inulin, amylase on starch, etc. A quantitative estimation of its hydrolyzing power

would afford some clue to the enzymatic activity of the stimulated fungus as compared with the normal growth, and experiments seem to show pretty clearly that there is greater proportional enzymatic activity in the former than in the latter. The same point is even more clearly illustrated by the various researches on the influence of chemical irritation upon alcoholic fermentation by yeasts. A variety of substances in minimal doses have been found to increase the fermentative activity of these fungi. While in such cases we are, of course, dealing with extracellular enzymes, it is not unreasonable to suppose that by analogy similar excitation follows with the intracellular enzymes. The intracellular enzymes are the ones which we may legitimately suppose to be connected more or less directly with the metabolic activity of the living organism. Now if anabolic activity is connected in any way with the reversible action of enzymes—as seems likely—we have here another link in the chain of evidence as to the real nature of chemical stimulation. We may hope in time to reduce it entirely to a question of enzyme formation. In order to do so we must devise more precise means for investigating the intracellular enzymes in the plants experimented upon and to determine if there is any quantitative difference as a result of stimulation. If it can be proved that this causes a relative increase in synthesizing enzymes in the fungus hyphæ a long step toward a more complete understanding of the processes will have been made. It should be acknowledged that at present such considerations are in a measure purely speculative, yet not speculative to the extent of being other than founded on the meager knowledge at hand. There is nothing improbable in such conclusions. The synthetic action of enzymes is a question which is more and more attracting the attention of the investigator, and while the results along these lines are comparatively new and relatively few in number, they are sufficiently conclusive to permit of a broad application of the principle involved. I will cite only one instance, and that in relation to an extracellular enzyme, where isomaltose has been synthesized from glucose by the action of maltase and, further, where the same enzyme was utilized in the synthesis of the glucoside amygdalin. Granting then that we may have in enzymes active agents in the constructive work of the organism, it is possible to understand how an increase in enzymatic activity could explain many of the phenomena connected with the response to chemical irritation.

There still remains, of course, the most fundamental question why and in what manner the specific irritants used affect the quantitative and even perhaps the qualitative formation of enzymes, and here there is no ready or sufficient answer to give. At first glance it does not appear to be connected with their dissociation in weak solutions, for non-electrolytes like morphine give a reaction as well as dissociable salts, although it is to be remarked that the concentration required with the former is many times greater than with the latter. If, as is believed by some, the *poisonous* action of salts depends on the degree of their dissociation, it is probably equally true that the stimulative action of minimal doses of these same salts is influenced by this same factor. But this assumption does not dispose of the large class of non-electrolytic poisons (and consequently stimulants), although I venture to suggest that the introduction of such substances into the sphere of protoplasmic activity may result in the formation by the protoplasm of by-products which are dissociable poisonous substances. Such an explanation would help to account for the large doses

of non-electrolytes which are necessary to produce a reaction on the plant organism.

There seems to be a much greater universality in the manner of the response to stimulation by poisons than in their actual toxic effect, a fact that has already been noticed, and for that reason I am strongly inclined to the opinion that the former does not depend upon the particular form which the latter may take, and so the increased enzymatic action may be considered to be a general phenomenon connected with this class of response.

There at once suggest themselves many very interesting problems in regard to the relation of chemical stimulation to morbid hypertrophies—using the word in its broadest sense—in higher plants, and also to what might be called the normal hypertrophies which ensue in the tissues of the ovary wall and surrounding parts after fertilization, without touching on the great question of the development of the fertilized egg itself.

In a previous address before this section, attention was called to the possible enzymatic changes induced by untoward chemical stimulation of the germ cells of certain plants and the results of this stimulation on the offspring. In the light of my own acquaintance with the question of chemical stimulation I see nothing improbable in such a point of view, even though we can not prove it at present.

There are many other considerations in connection with the question which might be profitably discussed and I am aware that I have really touched upon one side of the problem only, practically neglecting the morphogenic influence of chemical stimulants, but sufficient time has already been consumed and to open up new topics would be but to strain your patience further. The point which I have endeavored to develop and which I here repeat is that the chemical stimulants which have been discussed produce their effect indirectly and the nature of the response appears to be one of the increase of constructive enzymatic action over that which would take place under normal conditions from an equal and similar food supply.

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PUBLIC LECTURES AT THE HARVARD MEDICAL SCHOOL

The faculty of medicine of Harvard University offers a course of free public lectures, to be given at the Medical School, Longwood Avenue, Boston, Saturday evenings at 8, and Sunday afternoons at 4, beginning January 2, and ending April 30, 1910. Doors will be closed at five minutes past the hour. No tickets are required. Following is a list of the lectures and their subjects, with dates:

January 2—"The Influence of Mental and Muscular Work on Nutritive Processes" (illustrated), by Dr. F. P. Benedict.

January 8—"The Story of Vaccination," by Dr. M. J. Rosenau.

January 9—"What the Public should know about Patent Medicines," by Dr. M. V. Tyrode.

January 15—"Clean Milk" (illustrated), by Dr. Calvin G. Page.

January 16—"The Growth of School Children and its Relation to Disease," by Dr. W. T. Porter.

January 22—"Sprains, Strains and Fractures: Simple Facts of Diagnosis and Treatment" (illustrated), by Dr. J. B. Blake.

January 23—"The Glands of Internal Secretion and their Relations to Health and Disease" (illustrated), by Dr. W. B. Cannon.

January 29—"Small-pox" (illustrated), by Dr. J. H. McCollom.

January 30—"Hearing and Speech," by Dr. C. J. Blake.

February 5—"Posture and Carriage as affected by School and Clothing," by Dr. R. W. Lovett.

February 6—"The Care of Infants with Special Reference to the Prevention of Disease," by Dr. Maynard Ladd.

February 12—"Voice Production," by Dr. J. Payson Clark.

February 13—"Nervous Diseases in Children," by Dr. W. N. Bullard.

February 19—"Uses of the Microscope," by Dr. H. C. Ernst.

February 20—"Laboratory Methods, with the Microscope and Otherwise," by Dr. J. L. Bremer.

February 26—"What the Public may Rightfully expect from the Dentist," by Dr. C. A. Brackett.

February 27—"How Tumors Look under the Microscope" (illustrated), by Dr. F. B. Mallory.

March 5—"Foot Discomfort: its Cause and Rational Treatment," by Dr. R. B. Osgood.

March 6—"The Care of the Skin in Health and Disease," by Dr. C. J. White.

March 12—"The Treatment of Surgical Tuberculosis," by Dr. E. H. Bradford.

March 13—"The Abdominal Emergencies and the Need of Early Recognition and Prompt Remedy," by Dr. M. H. Richardson.

March 19—"The Hygiene of Early Life," by Dr. T. M. Rotch.

March 20—"The Dietetics of Early Life," by Dr. C. H. Dunn.

March 26—"Poliomyelitis Anterior Acuta," by Dr. J. L. Morse.

March 27—"The Diagnosis of Acute Febrile Disease," by Dr. Henry Jackson.

April 2—"The Value and Uses of the X-ray," by Dr. Percy Brown.

April 3—"How to Gain or Lose Weight," by Dr. F. W. White.

April 9—"The Way and How of Breathing," by Dr. E. G. Martin.

April 10—"Personal Hygiene" (to women only), by Dr. C. M. Green.

April 16—"The Nature and Proportion of Cures in Insanity," by Dr. E. E. Southard.

April 17—"Insanity and Modern Civilization," by Dr. F. H. Packard.

April 23—"Medical Advertisements and Kindred Subjects" (to men only), by Dr. Abner Post.

April 24—"On the Etiology of Certain Diseases Peculiar to the Tropics," by Dr. E. E. Tyzzer.

April 30—"The Healthy Man and his Bacteria," by Dr. A. M. Worthington.

THE CARNEGIE FOUNDATION FOR THE ADVANCEMENT OF TEACHING

AT the meeting of the trustees of the foundation, held on November 17, the rules for the granting of retiring allowances were amended so as to recognize service as an instructor in the retirement on the basis of age or disability, and the right to retirement for professors under sixty-five years of age with a minimum of service of twenty-five years was restricted to cases of disability. The retiring allowances of widows of professors who have served twenty-five years are retained.

The amended rules read as follows:

RULE I

Any person sixty-five years of age who has had not less than fifteen years of service as a professor, or not less than twenty-five years of service as instructor[1] or as instructor and professor, and who is at the time a professor or an instructor in an accepted institution, shall be entitled to an annual retiring allowance computed as follows:

(*a*) For an active pay of twelve hundred dollars or less, an allowance of one thousand dollars, provided no retiring allowance shall exceed ninety per cent. of the active pay.

(*b*) For an active pay greater than twelve hundred dollars the retiring allowance shall equal one thousand dollars, increased by fifty dollars for each one hundred dollars of active pay in excess of twelve hundred dollars.

(*c*) No retiring allowance shall exceed four thousand dollars.

Computed by the formula: $R = A/2 + 400$, where R = annual retiring allowance and A = active pay.

RULE II

Any person who has had twenty-five years of service as a professor or thirty years of service as professor and instructor, and who is at the time either a professor or an instructor in an accepted institution, shall, in the case of disability unfitting him for the work of a teacher as shown by medical examination, be entitled to a retiring allowance computed as follows:

[1] An instructor is held to be a college or university teacher to whom is assigned independent teaching or the conduct of laboratory work or of classes under the direction or supervision of a professor or head of a department. The term is not intended to include demonstrators, mechanicians, laboratory helpers or other assistants who are not charged with the responsibility for the conduct of college classes, nor is it held to include those who give any considerable part of their time to gainful occupations other than college teaching. The foundation reserves the right to decide in all doubtful cases what constitutes service as an instructor.

(*a*) For an active pay of twelve hundred dollars or less, a retiring allowance of eight hundred dollars, provided that no retiring allowance shall exceed eighty per cent. of the active pay.

(*b*) For an active pay greater than twelve hundred dollars, the retiring allowance shall equal eight hundred dollars, increased by forty dollars for each one hundred dollars in excess of twelve hundred dollars.

(*c*) For each additional year of service above twenty-five for a professor, or above thirty for an instructor, the retiring allowance shall be increased by one per cent. of the active pay.

(*d*) No retiring allowance shall exceed four thousand dollars.

Computed by the formula: $R = A/100(b + 15) + 320$, where R = retiring allowance, A = active pay and b = number of years of service.

RULE III

A widow who has been for ten years the wife of a teacher, who at the time of his death was in receipt of a retiring allowance, or who at the time of his death was eligible to a retiring allowance, or who had had twenty-five years of service as a professor or thirty years of service as an instructor and professor, shall receive as a pension one half of the retiring allowance to which her husband was entitled under rule I. or would have been entitled under rule II. in case of disability.

RULE IV

In the preceding rules, years of leave of absence are to be counted as years of service, but not exceeding one year in seven. Librarians, registrars, recorders and administrative officers of long tenure whose salaries may be classed with those of professors and assistant professors, are considered eligible to the benefits of a retiring allowance.

RULE V

Teachers in the professional departments of universities whose principal work is outside the profession of teaching are not included.

RULE VI

The benefits of the foundation shall not be available to those whose active service ceased before April 16, 1905, the date of Mr. Carnegie's original letter to the trustees.

RULE VII

In counting years of service toward a retiring allowance it is not necessary that the entire service shall have been given in institutions upon the accepted list of the foundation, but only years of service in an institution of higher education will be accepted as an equivalent.

RULE VIII

In no case shall any allowance be paid to a teacher who continues to give the whole or a part of his time to the work of teaching as a member of the instructing staff of any institution.

RULE IX

The Carnegie Foundation for the Advancement of Teaching retains the power to alter these rules in such manner as experience may indicate as desirable for the benefit of the whole body of teachers.

SCIENTIFIC NOTES AND NEWS

READERS of SCIENCE will have learned with regret of the circumstances leading to the retirement of Mr. Gifford Pinchot from the direction of the Forest Service.

DR. C. F. CHANDLER, since 1864 professor of chemistry in Columbia University, will retire from active service at the close of the present academic year. The trustees have passed a resolution expressing their high appreciation of his services to the university.

PROFESSOR THEODORE W. RICHARDS, of Harvard University, has been reappointed research associate of the Carnegie Institution of Washington, having received a new grant of $2,500 for the continuation of his researches on atomic weights and other physico-chemical constants.

PROFESSOR G. P. BAXTER has also been reappointed a research associate of the institution and a grant of $1,000 has been made to him for the continuation of investigations upon atomic weights.

PROFESSOR E. J. WILCZYNSKI, of the University of Illinois, has been awarded a prize of eight hundred franks by the Royal Academy of Science, Letters and Fine Arts of Belgium, for his memoir on "The General Theory of Congruences." This prize was announced some time ago by the academy for the most noteworthy development of some phase of the application of differential geometry to ruled space.

DR. F. W. PUTNAM, emeritus professor of American archeology and ethnology at Harvard University, has been appointed honorary academician of the Museum of the National University of La Plata in the section of the natural sciences.

PROFESSOR KARL RUNGE, of the University of Göttingen, Kaiser Wilhelm professor at Columbia University, and Professor Otto Jespersen, visiting professor from the University of Copenhagen, have been given the degrees of D.Sc. and D.Litt., respectively, by Columbia University.

IN recognition of the services rendered by him in the reform of medical education in Hungary, and of the active interest taken by him in the International Medical Congress held last year at Budapest, the medical faculty of the university of that city has conferred on Count Albert Apponyi, the minister of education, the honorary degree of doctor of medicine.

MR. JOHN A. VOGELSON has been appointed chief of the Philadelphia Bureau of Health in succession to Dr. A. C. Abbott, who resigned some months ago.

MR. C. H. T. TOWNSEND has been given leave of absence by the Department of Agriculture for eighteen months to inaugurate an entomological service for the Peruvian government.

THE American Nature-study Society has elected Professor O. W. Caldwell, of the University of Chicago, president of the society, and Professor F. L. Charles, of the University of Illinois, secretary and editor of *The Nature-study Review*. The office of that journal will be removed from New York City to Urbana, Ill.

DR. AUSTIN M. PATTERSON was elected editor and M. John J. Miller associate editor, of *Chemical Abstracts* at the Boston meeting of the American Chemical Society, Professor W. A. Noyes, of the University of Illinois retiring. The office of this publication was removed last August from Illinois to Ohio State University, where it has since been in charge of Dr. Patterson as associate editor.

DR. WILLIS L. MOORE, chief of the Weather Bureau, delivered a lecture on December 27 in the assembly room of the Automobile Club of America, New York City, on "The Work of the Weather Bureau in Relation to Aeronautics."

DR. E. L. THORNDIKE, professor of educational psychology in Teachers College, Columbia University, gave on January 11 an address before the Middletown Scientific Association on "Experimental Studies in Animal Intelligence."

SIR ERNEST SHACKLETON lectured in Rome on his Antarctic expedition on January 3. Among those present were the king and queen.

DR. G. BOWDLER SHARPE, assistant keeper in the department of zoology of the British Natural History Museum, and eminent as an ornithologist, died on December 25, at the age of sixty-two years.

PROFESSOR H. H. GIGLIONE, director of the Royal Museum of Natural History and professor of zoology at Florence, known as an ethnologist as well as a zoologist, died on December 20, at the age of sixty-six years.

DR. EDOUARD BRISSAUD, professor in the Paris School of Medicine and well known for his work in pathological anatomy and medicine, has died at the age of fifty-two years.

A BILL has been introduced into the house of representatives making the present Bureau of Education a Department of Education with a secretary in the Cabinet.

THE offices of the Bureau of American Ethnology were on January 1, 1910, transferred from the Adams Building on F Street to quarters in the Smithsonian building. Mr. F. W. Hodge on that date assumed charge of the bureau with the title of ethnologist-in-charge.

THE path of Halley's comet has been added to the planetarium in the Foyer of the American Museum of Natural History, and its position in the solar system will be indicated daily during the next few months, while the comet is visible to the unaided eye.

THE New York Aquarium had a greater number of visitors during the year 1909 than

ever before, the attendance being 3,803,501, an average of 10,417 a day. These figures show that the aquarium has a greater patronage by the public than all the other museums of the city, including the Zoological Park, combined. These figures are unequalled by those of any other museum in the world of which statistics are available.

THE Geological Survey's report on the production of copper in 1908, prepared by B. S. Butler, is now ready for distribution. The mine production, smelter output and refinery production in 1908 exceeded those of 1907. The production in 1908 by smelters from copper-bearing material mined in the United States was 942,570,721 pounds, the largest in the history of the industry. The production in 1906, the next largest, was 917,805,682 pounds; that for 1907 was 868,996,491 pounds. The world's production of copper in 1908 was 1,667,098,000 pounds, so that the United States contributed considerably more than half the total product of the metal. The exports of refined copper were 618,613,842 pounds, the largest amount recorded; the imports were 218,705,487 pounds, mostly from Mexico, Canada and Peru. The domestic consumption of new copper in 1908 was 480,000,000 pounds; of old copper 23,000,000 pounds, making the total domestic consumption 503,000,000 pounds, against 547,000,000 pounds in 1907. The stock on hand January 1, 1908, was 125,745,796 pounds; on January 1, 1909, it was 121,876,759 pounds. The average quoted price of electrolytic copper at New York in 1908 was 13.2 cents a pound. In 1907 the price was 20 cents a pound. The commercial conditions during the year were very stable, the variations in monthly average price covering a range of only 1.54 cents, as compared with 11.90 cents in 1907. A notable feature of the industry was a decrease in cost of production due to improvements in methods and the increased efficiency of labor. Arizona ranks first in the production of copper, Montana, Michigan, Utah, California, Tennessee, Colorado, Nevada, Idaho and New Mexico following in the order named. According to the smelter returns the three leading copper states—Arizona, Montana and Michigan—produced 81 per cent. of the total output in 1908.

THE London *Times* states that the exhibition which is to open in December next at Allahabad will be the largest ever held in India. While essentially non-official in character, the strong executive committee in charge enjoys the support of the local government, which is itself managing the agricultural, forest and educational courts. Sir John Hewett, the present lieutenant-governor, who was lately commercial member in the governor-general's council, and other government officials are giving their aid in every legitimate way. The exhibition committee specially desire machinery and demonstrating processes for the agricultural, engineering and textile courts. As is stated in the preliminary prospectus, "the main object of the exhibition is to encourage the arts and industries of the united provinces by displaying products and methods of production and by introducing from other countries such commodities as are required to supplement indigenous productions." The first aviation meeting in the east will be held in connection with the exhibition.

IN the course of its last session the state legislature of North Dakota provided for the establishment of a biological station on the shore of Devils Lake. The bill places the station under the direction and control of the trustees of the University of North Dakota, and provides that the biological staff of the university shall direct the work of the station. The bill further states that, "It shall be the duty of the staff of said station, as directors thereof, to study the animals and plants in Devils Lake and other portions of North Dakota with reference to the problem of restocking and cultivating fish in Devils Lake and in any other waters of the state, especially those of an alkaline character, such as Devils Lake; to study and make collections of any animals and plants in North Dakota that have commercial and scientific value." The station is housed in a commodious, artistic building of cobble and concrete. The equipment, now being procured, will consist of boats, dredges, seines, pumps and all the

varied apparatus necessary for the study of small animals and plant forms. The laboratories will be equipped, some of them for general students of biology and some for special research. Salt water from the lake and fresh water from other sources will provide for the aquaria and other receptacles. Investigations already made show that Devils Lake swarms with life in great numbers though not in great variety of species. North Dakota has a rare opportunity to study the biological stages of variation, adaptation and isolation, and it is the purpose of this new station to provide for biological students and to offer facilities for trained investigators who may desire to carry on research under such conditions as exist in that region.

THE *Auk* states that the Museum of Comparative Zoology at Harvard University has received during the past year the most noteworthy accessions to its bird collection in its history. These include, as the most important, the E. A. and O. Bangs collection, containing approximately 24,000 skins, chiefly from North and middle America and the West Indies. Most of those from Central America were taken by Mr. Wilmot W. Brown and Mr. C. F. Underwood, well known as intelligent and energetic collectors, the former noted for his skill in preparing skins of birds and mammals. The specimens have been determined by Mr. Outram Bangs, with the assistance of Mr. Ridgway and Dr. Richmond. They also include the types of the many new forms described in recent years by Mr. Bangs. Another gift of unusual importance consists of several thousand specimens from the interior of central and western China, presented by Mr. John E. Thayer. A collection of over 3,000 skins collected in Palestine have been acquired by purchase. A considerable number of other skins and mounted specimens of unusual interest have also been acquired, by gift or purchase, from other sources.

UNIVERSITY AND EDUCATIONAL NEWS

THE late Arthur Hill, regent of the University of Michigan, has bequeathed $200,000 to the university for the erection of an auditorium.

THE gift of $650,000 by Mrs. Russell Sage to pay for the Hillhouse property, of Yale University, will release a considerable sum which, according to the correspondent in the N. Y. *Evening Post,* will probably be used for work in biology.

THE building of the University College of Medicine, at Richmond, Va., was destroyed by fire on January 6, entailing a loss estimated at $200,000.

THE board of trustees of the Massachusetts Agricultural College having received propositions from the Boston & Albany Railroad and also from electric railroads centering in Springfield to run agricultural educational trains over their respective lines, it has been voted, "That this board will heartily cooperate with these railroads, the Board of Agriculture, the Chamber of Commerce and other organizations in the state to further the development and success of this project. To this end it will tender the services of its teachers and experts and place at the disposal of these railroads such equipment and apparatus as may be required."

AT the annual meeting of the governors of the Nottingham University College on December 22 it was announced that it was the intention of the council to develop immediately a scheme for submission to the court of governors that application might be made for a full charter, so that Nottingham College would become a degree-conferring university.

DR. C. J. KEYSER has been appointed head of the department of mathematics of Columbia University, to succeed Professor J. H. Van Amringe, who retires from active service at the close of the academic year.

DR. J. L. SIMONSEN has resigned his position as assistant lecturer and demonstrator in chemistry in Manchester University to accept the chair of chemistry in the University of Madras. Mr. Alfred Holt, M.A., D.Sc., has been appointed as his successor.

Mr. L. A. Borradaile, M.A., of Selwyn College, has been appointed university lecturer in zoology at Cambridge University.

DISCUSSION AND CORRESPONDENCE

COTTON ANTHRACNOSE

Since Dr. Atkinson's work on cotton anthracnose, 1890–3, little has been done on this now important disease. Recent work here has brought out some very interesting points which in a way confirm some of Dr. Atkinson's theories in connection with the infection of seed and seedlings. Last winter while working with seed taken from a field where the disease occurred the previous summer, I found anthracnose occurring in a number of germination tests. This led me to search for the fungus in the tissue of the seed. I found that by taking bolls which were slightly diseased and mature it was an easy matter to find the fungus filaments beneath the seed coats and in the tissues of the cotyledons. The spores of the fungus are also readily found between the seed coats and the cotyledons of mature seed. Numerous inoculation experiments during the past summer show that the fungus seems to prefer the seed and lint to other portions of the plant. In fact, in some cases the attack is confined to these parts, there being no sign of the disease on the walls of the bolls. In some cases where the bolls mature and the cotton opens out with no sign of disease other than slight discoloration of the lint, the fungus will be found on such lint and in the seed. Such seed, of course, when planted produce diseased seedlings and thus spread the disease. This season numerous outbreaks of anthracnose in various sections of this state have been traced to diseased seed. Some of these occurred where cotton had never been planted before. From an economic standpoint this phase of the problem seems to be very important. The south is now sustaining a loss of millions of dollars annually from anthracnose. It has been estimated that the state of Georgia loses over $14,000,000 annually and a very conservative estimate of the loss of South Carolina would be from $400,000 to $500,000 annually.

Since the twentieth of last July I have been unable to isolate the fungus from the fields where cotton was planted last year. From this it seems that a one year's rotation with disease-free seed might eliminate the disease.

Interesting results have also been obtained in reference to seed treatment, method of infection of the bolls, resistance of different varieties of cotton, breeding resistant strains, etc., all of which will be published at an early date in report of the South Carolina Experiment Station.

H. W. Barre,
Botanist

South Carolina Experiment Station
October 26, 1909

METAPHYSICS AND MENDELISM

There are reasons for regarding man as a chimpanzee on which an additional element, "manness," has been superposed. There you have man expressed or explained in terms of his anthropoid ancestor. The characters of a frog are undoubtedly latent in the frog's tadpole. What is to hinder, therefore, expressing or explaining the frog in terms of the tadpole by saying the tadpole carries the characters of the frog? The logic is sound in the statement that the tadpole contains frog factors or "frogness." The question is merely as to the helpfulness of sound logic used that way.[1]

The helpfulness of sound logic, aside from its use as a mental discipline, is usually based on its relevance to the matter under discussion. As regards the chimpanzee we shall doubtless all agree with the learned Californian if he will advance scientific proof that in homo-simian hybrids "chimpanzeeness" and "manness" behave toward each other in Mendelian ratio; for it is Mendelian inheritance, it must be remembered, that the English scientists are talking about. If the tadpole contained the potentiality of developing either into a frog or, let us say, a salamander, according to circumstances under experimental control, we might consider "frogness" as a factor, the presence or absence of which would have a determinative influence in development.

[1] "The Hypothesis of 'Presence and Absence' in Mendelian Inheritance," W. E. Ritter, Science, September 17, 1909.

In other words, the allusions to the frog and chimpanzee, true or otherwise, are not particularly illuminating in a discussion of Mendelism because there is involved no feature of dominance nor alternation of characters.

In Mr. Punnett's original statement of what is known as the Cuénot theory:[3]

There are but two relations into which the unsplittable unit character can enter with the individual. It may be present or it may be absent and no third relation can be conceived. From this we are led to ask whether the hypothesis can be brought into any simple relation with the phenomenon of dominance. Is dominance the outcome of the presence of the given factor, and recessiveness the condition implied by its absence? At present we can only say that such a point of view is not at variance with the great majority of cases hitherto worked out. Whether the few instances which now seem contradictory will ultimately fall into line, future work alone can decide.

Nothing very cryptic or very dogmatic about that. In speaking of "roseness," "peaness," etc., Mr. Punnett has merely framed a convenient and probably temporary handle to grasp a difficult subject in order the better to inspect it. We owe him a vote of thanks, that, instead of christening his conceptions with newly coined words dug from the dusty depths of the Greek lexicon, he has rather chosen to emphasize their temporary character by Englishing them, lest others should read into his statements a concreteness he manifestly wishes to avoid.

The writer is of those who believe that the dangerous facility with which the facts of Mendelism fall into categories and A-B-C notations is illusory and that the matter is more complicated than those would have us think who have allowed themselves to be entangled in all-explaining formulæ. Yet working hypotheses we must have in order to advance, and none suggested so far is any more usable, certainly none more lucid, than the one Professor Ritter finds so contaminated with metaphysics.

J. F. ABBOTT

ST. LOUIS, MO.,
September 29, 1909

[3] R. C. Punnett, "Mendelism," 1907.

HYDROGEN POLYSULPHIDE AS A REDUCING AGENT

I SHOULD like to correct a clerical error in the account I gave a few months ago[1] of my investigation of the reducing action of hydrogen polysulphide. The statement "it may be used at the ordinary temperature, dissolved in ionizing solvents, such as water or alcohol, or in non-ionizing media, such as carbon bisulphide" should read "it may be used at the ordinary temperature, *for the reduction of substances* dissolved in, etc."

As is well known, the polysulphide is practically insoluble in water and alcohol.

ALFRED TINGLE

LABORATORY OF THE IMPERIAL CHINESE
PEI YANG MINT, TIENTSIN,
October 10, 1909

[1] SCIENCE, XXX., 158 (July 30, 1909).

SCIENTIFIC BOOKS

Landmarks of Botanical History. By EDWARD LEE GREENE. Smithsonian Miscellaneous Collections (Vol. 54), 1909.

We have had many histories of botany, each of which has added somewhat to our knowledge of the growth of the science and of the men who have been its chief workers, or they have given us a new point of view so that we have been able to see how botany has grown and developed from its crude beginnings to the present. In Dr. Greene's book we have another attempt to set forth the matter in a new light, and at the outset it may be said that few men could bring to the task better ability, training and preparation. Nor are there many men who can command equal library facilities, for Dr. Greene's unrivaled private library of the earlier botanical works is supplemented by the Congressional Library, to which as an attaché of the Smithsonian Institution he has had the freest access. This happy coincidence with the unusual freedom from official duties afforded by his position, and a persevering industry, have conspired to favor the production of a monumental work.

In choosing for his title the word "landmarks" the author indicated something as to

what his treatment of the problem was to be. He has chosen to bring before his readers the lives and teachings of botanists, and necessarily he must choose those who have contributed to the upbuilding of the science. This treatment is in sharp contrast with the chronological method in which each botanist is taken up in his proper place, and his various publications cited, much as they are in a publisher's descriptive book list. It is also quite different from the treatment made familiar to us by the well-known history of botany written by the late Professor Sachs, in which the development of each department of botany is traced consecutively and consistently. In the latter treatment the subject is so emphasized that the men themselves fall somewhat into the shadow; we think of how this or that part of the science developed, but largely overlook the personal element as represented by the men by whose labors the development took place. By the one method we have a work on botany in which the present condition of each part of the science is accurately given, and we are shown by what steps this condition was reached. In this treatment the botanists are but the workmen who have helped to build the edifice of science; they are important only as they have added stones to its structure, and while the historian mentions their names, these are wholly secondary, and may be forgotten in our admiration of the aggregate result of their work. By the other method we are brought to consider the workmen who have labored upon the edifice; how they worked; how they succeeded in their endeavors; how they failed here and there, and why they failed, as well as why they succeeded. By this treatment we learn not only what progress was made in the upbuilding of the science, but also *how* it was made. For the botanist who wishes merely to know the material of the edifice, the method of Sachs is preferred, but for the investigator who desires to know the conditions under which his predecessors did their work the other method is indispensable.

As indicated above, Dr. Greene has chosen to write his history so as to place the emphasis first upon the men who have worked in botany. It is thus a very human book, and as one reads the biographies of the men he has selected a vivid picture is presented of their lives and their labors, as well as their environment. As one reads he gets some idea of the atmosphere in which men lived, and he appreciates all the more the difficulties they encountered, and the meaning of success in their particular environment.

It is understood that this history—"Landmarks"— will cover several volumes, and certainly if one may judge of the succeeding volumes by the first there can be no question as to the desirability of continuing the work as it has been begun. It opens with a most readable and suggestive preface, in which the author gives his definition of botany—as that science "that occupies itself with the contemplation of plant as related to plant, and with the whole vegetable kingdom as viewed philosophically—not economically or commercially—in its relation to the mineral on the one hand, and to the animal on the other." It is, however, distinctly set forth that to the botanist all matters relating to plants must be of interest, and he has clearly no sympathy with those who would close their eyes to the industrial relations of the science. He goes so far, even, as to include as "essentially botanical" those philosophic ideas, though crude or erroneous, about the vegetable kingdom as a whole or in part which may occur to "the farmer, the woodsman and the primitive pharmacist" and others who have much to do with plants industrially. With this liberal interpretation no broadly trained botanist will find fault, nor should the workers in agriculture, forestry and other allied subjects object to this inclusion of the philosophical aspects of these phases of plant study.

The introduction, covering about thirty pages and devoted to The Philosophy of Botanical History, is well worth reading, since it is full of suggestions, some of which we should like to quote if there were space to do so. The root-gatherers ("Rhizotomi") "mostly illiterate men and quacks," who preceded Aristotle and Theophrastus, receive liberal treatment in a short chapter. This is followed (chapter

II.) by nearly a hundred pages devoted to Theophrastus of Eresos, one of the most instructive parts of the book. The treatment here illustrates the author's method, who says (p. 60): "In our study of this maker of the first Landmark in the History of Botany the main object must be that of discovering in what ways, under what limitations, and yet how well, he accomplished the placing knowledge of plant life and form upon the list of the sciences." Accordingly, a dozen pages are given to a discussion of his method, which is in fact continued through nearly thirty pages more under the subtitles Organography and Anthology. After this Phytography (5 pages) leads to Taxonomy (20 pages) and Dendrology (8 pages). The chapter closes with a recapitulation in which the author shows that Theophrastus "is the father of the Science as we now have and hold it."

The short chapter on the Greeks and Romans after Theophrastus (enumerating Nicander, Cato, Varro, Virgil, Columella, Dioscorides, Pliny and Galen) leads to a still shorter one on the botany of the middle ages, the author remarking in passing that "the period has no apparent landmarks of botanical history."

Otho Brunfels (chapter V.), who is characterized as "first in point of time among the German botanical reformers of the sixteenth century," leads the way to Leonhardus Fuchsius and Hieronymus Tragus, to each of whom a chapter is assigned. The short chapter (VIII.) devoted to Euricius Cordus leads naturally to the following (IX.) on Valerius Cordus, the son, "hitherto almost unknown except by name." This closing chapter of the volume will be read with keen interest by every botanist, who will learn here for the first time, perhaps, of this brilliant botanist whose death when but twenty-nine years of age closed a life of much achievement and still greater promise. To have rescued the name of Cordus and his work from oblivion was a worthy labor, and most zealously has Dr. Greene carried it out. He shows that Cordus formulated plans for his plant descriptions, and that with these he redescribed "some of the best known and best described plants of Dioscorides," which is characterized as "the boldest innovation that was made by any botanist of the whole sixteenth century."

The "Landmarks of Botanical History" will certainly be of the greatest value to botanists the world over, since it presents the subject in a new light and from a different point of view. We shall all pray for the continued health and strength of the author, and that opportunity may be afforded him of completing the work to which he has set his hand.

CHARLES E. BESSEY
THE UNIVERSITY OF NEBRASKA

The Moon in Modern Astronomy. By PH. FAUTH. With an introduction by J. E. GORE, F.R.A.S Pp. 160 with 66 illustrations. New York, D. Van Nostrand Company. 1909.

This attractive book gives a very interesting account of the principal features visible on the moon's surface and it embodies the results of over twenty years of careful study with small telescopes. The subject is treated in an historical manner, especial attention being given to the early maps of Lohrmann, Mädler and Schmidt. M. Fauth shows that photographic processes have not materially added to our knowledge of lunar conditions. In fixing the relative positions of the larger surface features photographs are more accurate than maps made from eye observations, but for the study of minute detail visual observations, even if made with relatively small telescopes, are superior to the best photographs.

The most conspicuous features of the moon's surface are the so-called "craters." These have heretofore been described as "cup-shaped" mountains and as resembling but greatly exceeding the great volcanic craters of the earth. M. Fauth shows that this conception of the lunar "craters" is erroneous, that they are more like shallow dishes, and could more appropriately be called "walled-plains." He shows by figures and by diagrams that in many cases the crater is "so incredibly shallow that the eye of an observer on the crest would hardly be able to see the crest on

the opposite side, because the depression is so slight that the curvature of the moon's surface covers the opposite wall." Or again "A dessert dish five inches in diameter (without the border) and less than a quarter of an inch in depth has twice as deep a cavity, proportionally, as the deepest of these depressions."

M. Fauth considers in detail the various theories that have been advanced to account for the origin of these peculiar features of the lunar surface and rejects them all as unsatisfactory. Yet it can not be said that he has disproved the volcanic theory, although he has certainly pointed out many difficulties in the generally accepted idea of that theory. But the theory that he advances in its place, that the moon's surface is covered by a deep layer of ice, will not be accepted without convincing proof.

The book is well printed and illustrated and is well worth reading by those who take an interest in the moon.

C. L. P.

In Starland with a Three-inch Telescope. By WILLIAM TYLER OLCOTT. Pp. 146 with many diagrams. New York, G. P. Putnam's Sons. 1909.

This is a convenient hand-book or guide for the amateur astronomer. A three-inch telescope is too small to show any planetary detail and the owner of such an instrument is practically limited to the study of the moon and of a small number of the brighter double stars. To a description of these objects, therefore, the book is confined.

Only the constellations visible in the latitude of the New England and Middle States are included and these constellations are divided into four groups, corresponding to the seasons of the year in which they are visible. For each constellation a clear and simple page map is given and on this map are marked the positions of the interesting double stars. Facing each map is a printed page, which gives the necessary details for finding and observing these objects.

The moon is treated in a similar manner, eight diagrams of different phases being given. These show the principal features only and should be of great assistance to the student of lunar detail.

The book is well printed, the maps and diagrams well designed and executed, and the little volume is admirably adapted to encourage the study of the heavens.

C. L. P.

SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Experimental Zoology, Vol. VII., No. 2, contains the following papers: "Wound Reparation and Polarity in Tentacles of Actinians," by Herbert W. Rand. A distal cut end of a tentacle of the large actinian, *Condylactis*, is immediately closed by muscular contraction and remains so during a slow process of structural repair which eventually replaces the muscular contraction. The distal cut end of an excised fragment of tentacle behaves similarly, but a proximal cut end does not close. In the conspicuously different behaviors of proximal and distal cut ends, and in responses to tactile stimulation, the tentacle shows a marked polarity which can not be accounted for upon the basis of its known structure. "A Biological and Cytological Study of Sex Determination in Phylloxerans and Aphids," by T. H. Morgan. An analysis of the behavior of the sex chromosomes in phylloxerans in connection with the behavior of the sex chromosomes in other hemiptera leads to the conclusion that these chromosomes can not be male and female determining as such, but that they are identical in all respects. Two alternative views offer themselves if this analysis is correct. Either sex is determined quantitatively by the amount of elimination contained in the fertilized egg—a view advanced by the author in 1907 and since adopted by Wilson and by Castle in a modified form—or else the presence or absence of the sex chromosomes are associated with other profound, invisible differences in the two classes of spermatozoa. It is difficult to decide at present between these alternatives, but the facts here recorded for the phylloxerans favor the interpretation that the visible chromosomal differences in the two

classes of spermatozoa are associated with more profound differences in the sperm and that it is these differences rather than the difference in quantity alone that have a determinative influence in sex determination. An examination of almost 10,000 male and female eggs of *P. caryæcaulis* shows that the male eggs occur about five times as often as the female eggs. A study of the output of each stem-mother shows that in some cases all of her progeny are males, in other cases all females, and in most cases both males and females with a preponderance of males. The results are obviously not connected with chance combinations of chromosomes, but definite "tendencies" exist in certain individuals that follow one or the other alternative. These tendencies might seem to be the result of external factors, but nothing was discovered in the history of the individuals that favors such an interpretation; although the possibility of such an effect must be granted. The author's general conclusion is summed up in the statement that the quantitative interpretation of sex-determination is only the first rude approximation to a solution. The facts suggest that the visible quantitative differences are associated with more profound changes and the facts described for the phylloxeran egg give some indication of the nature of those changes; for, the sex chromosomes seem rather to follow sex than to be its sole cause.

"Factors of Form Regulation in *Harenactis attenuata,* III., Regulation in Rings," by C. M. Child. Under certain conditions short cylindrical pieces from the body of the actinian, *Harenactis attenuata* form "rings" by the union of oral and aboral ends. Such rings may give rise to one or several more or less radially symmetrical groups of tentacles in the region of union.

THE FIRST CRUISE OF THE "CARNEGIE" AND HER EQUIPMENT[1]

THE *Carnegie,* engaged in a magnetic survey of the oceans under the direction of the department of terrestrial magnetism of the Carnegie Institution of Washington, entered on her first cruise August 21 last. As may be recalled, this is the first vessel in which the attempt has been made to exclude practically all materials affecting the compass needle. Hence the magnetic data secured on her can be made immediately available, it being now unnecessary to await the determination of troublesome and more or less uncertain deviation corrections.

The tests made at Gardiner's Bay, Long Island, August 21 to September 2, and at Falmouth, England, have demonstrated conclusively that no correction of whatever kind need be applied to the *Carnegie* results. The following table will show the close agreement in the values of the three magnetic elements obtained on the various headings during the swings at Gardiner's Bay:

Ship's Head	Magnetic Declination (Variation of the Compass)	Magnetic Dip.	Horizontal Magnetic Intensity (C.G.S. Units)
N.	11°25′ W.	72°01′	.1825
NE.	26	07	23
E.	28	07	25
SE.	22	07	25
S.	22	05	27
SW.	19	02	25
W.	21	05	22
NW.	27	11	23
Mean	11 24	72 06	.1825

Nothing could be more satisfactory than this exhibit of the fulfillment of the requirements as to non-magnetic conditions at the places where the instruments are mounted.

The observations made on the trip from New London, Conn., to St. Johns, Newfoundland, and from there to Falmouth, England, during the severe October gales afforded ample opportunity for trying out the observational appliances, and these stood the tests put upon them, during the exceptionally adverse conditions, even beyond expectations. A large part of the instrumental equipment was especially designed and constructed in the workshop of the Department of Terrestrial Magnetism.

In brief, it may be confidently asserted that

[1] Abstract of paper presented before the Philosophical Society of Washington, November 20, 1909, by Dr. L. A. Bauer, of the Carnegie Institution of Washington.

ocean magnetic work is now on a stage of perfection not hitherto reached, permitting obtaining useful data not only expeditiously but also with all necessary accuracy both as regards practical and scientific demands.

The introduction of circular observatories in which the magnetic instruments are mounted has proved of great advantage. The domes being revolvable, it is possible to direct an open panel to any part of the skies, thus permitting astronomical and magnetic observations being made with full protection to the observer and the instrument from wind and weather. Hitherto all such observations have had to be made on an open bridge.

Another important feature of the research work on this unique vessel is the developing and perfecting of a producer gas engine for auxiliary marine propulsion. The *Carnegie* has a non-magnetic plant of this kind of 150 horse power, sufficient to drive her at six knots in calm weather, or at about 144 knots per day at a total cost for coal consumed during the day of but seven dollars. Such difficulties as have been encountered thus far are mainly due to the non-magnetic metals which have had to be so largely employed. However, these difficulties are being successfully solved one by one. As a matter of fact, the *Carnegie* has entered and left every port thus far, under bare poles, with the aid of her auxiliary power and so likewise the vessel was swung during the trial tests at Gardiner's Bay and Falmouth, using only the auxiliary power. This vessel is the first sea-going one having such a plant.

The next table gives the results of the magnetic observations up to Falmouth.

The last three columns show the average errors of the best magnetic charts at present available. Glancing over them the following conclusions may be drawn:

1. From Long Island to some point off Newfoundland the charts used by mariners show too small westerly magnetic declinations (variations of the compass) by about one degree in the maximum, thereafter and continuing to England, the error changes sign, indicating that the charts give too large west magnetic declination, the maximum error being nearly one degree. Were there not such a systematic run in the errors they would not be of great importance to navigation, but as the sign is the same for great distances the general effect would be, in the present instance, to set the course of a vessel (when reliance must be put solely upon the compass and the log) always towards Newfoundland, whether the vessel came from the east or the west.

Magnetic Results obtained on the "Carnegie," September–October 18, 1909, in the Atlantic Ocean

No.	Latitude N.	Longitude W. of Greenwich	Date 1909	Declination West	Dip. N.	Hor. Int. C.G.S. Units	Corrections of Charts		
							Declinations	Dip	Hor. Int. C.G.S. Units
	°	°		°	°		°		
1	41.1	72.2	Sep. 1	11.4	72.1	.183	+0.4	+0.2	—.002
2	41.0	71.1	13	12.3	72.0	.182	+0.4	+0.1	—.003
3	40.9	70.4	14	12.9	....	....	+0.8	....	....
4	40.7	69.4	14	12.8	71.7	.185	+0.3	—0.1	.000
5	40.7	68.9	15	13.9	71.9	.182	+0.6	+0.2	—.003
6	40.9	68.4	16	14.4	....	....	+0.6	....	....
7	41.3	66.4	17	16.2	71.9	.181	+0.6	0.0	.000
8	42.0	61.1	20	20.2	....	.177	+0.9	....	.000
9	42.5	61.2	21	20.8	....	.176	+0.9	....	+.003
10	42.8	60.8	21	21.4	72.5	.173	+1.1	+0.5	.000
11	43.8	58.9	22	23.7	72.7	.171	+1.1	+0.6	+.004
12	45.5	55.7	23	......	72.7	.169	....	+0.4	+.010
13	47.3	52.6	25	......	73.5	.158	....	+0.2	+.006
14	47.6	52.7	28	29.75	73.5	.159	0.0	—0.1	+.008
15	47.8	51.4	Oct. 3	30.4	....	....	0.0	....	....
16	48.2	50.4	3	......	73.5	.157	....	—0.2	+.008
17	48.4	48.0	4	31.8	....	....	+0.3	....	....
18	48.5	47.7	4	31.8	73.0	.161	+0.1	—0.1	+.011
19	48.7	46.5	5	31.8	....	....	—0.2	....	....
20	48.9	45.5	5	32.1	72.5	.161	—0.2	—0.2	+.010
21	49.6	37.5	7	......	71.2	.168	....	0.0	+.014
22	50.3	32.1	8	30.2	70.7	.171	—0.4	—0.3	+.014
23	50.6	28.8	9	29.0	....	....	—0.8	....	....
24	50.6	24.0	10	26.6	....	....	—0.8	....	....
25	50.6	22.2	10	......	69.2	.174	....	—0.3	+.008
26	50.5	19.2	11	24.5	....	....	—0.2	....	....
27	50.3	17.2	11	22.9	68.3	.180	—0.8	+0.1	+.006
28	49.9	11.9	12	20.3	67.4	.185	—0.6	+0.4	+.006
29	49.6	9.3	13	19.7	....	....	+0.2	....	....
30	49.5	7.5	13	18.6	66.3	.189	—0.1	+0.2	+.001
31	50.0	5.0	14	17.5	....	....	0.0	....	....
32	50.1	5.0	18	17.8	66.5	.187	+0.2	+0.2	—.002

No. 1 at Gardiner's Bay; No. 14 at St. Johns, N. F.; No. 32 in Falmouth Bay, England.

2. The chart errors in dip may amount to one half degree.

3. The chart values of the horizontal intensity are in general too low, the error amounting at times to nearly one tenth part.

4. A part of the errors found in the three magnetic elements are due to secular variation.

SPECIAL ARTICLES

THE PLEISTOCENE OF THE MISSOURI VALLEY

IN the course of his recent field studies for the Iowa Geological Survey in the Missouri Valley in western Iowa and eastern Nebraska, the writer was able to determine the following succession of Pleistocene formations:

1. The oldest drift sheet known in Iowa, to which the names pre-Kansan, sub-Aftonian, Albertan and Jerseyan have been applied, is exposed to a depth of more than fifteen feet, and may be traced along the foot of the bluffs for several miles on both sides of the Missouri near Omaha and Council Bluffs.

The terms pre-Kansan and sub-Aftonian have been applied merely to designate the relative position of this drift sheet. The Albertan deposit is not now regarded as a drift, and moreover neither the Albertan nor the uncertain Jerseyan can be correlated with the sub-Aftonian of Iowa. This drift has now been found in various parts of Iowa, Missouri and Nebraska, and probably in South Dakota, but nowhere does it reach such development as in the region under discussion.

Because of the great extent of this formation, and the fact that it can not be correlated with any named horizon, it is proposed that the name *Nebraskan* be applied to it.

The typical exposures above Florence and in South Omaha in Nebraska, and about four miles above Council Bluffs in Iowa, have been noticed by geologists, but the deposit was referred to the Kansan, or was identified as Carboniferous shale.

The Nebraskan consists of a dark, bluish-black tough joint clay which breaks up into very small blocks upon exposure to the air, and through which are scattered small boulders and pebbles which are also mostly dark in color.

2. Upon the Nebraskan, but sharply separated from it, rests a deposit of Aftonian sands and gravels. This is very commonly exposed on both sides of the river and reaches a thickness of more than 30 feet. In its lower part the gravels are often cemented into conglomerates to a depth of several feet.

Fine exposures of Nebraskan and Aftonian (the latter consisting of gravels, sands and sometimes silt) occur on both sides of the river, but those which appear along the Chicago Northwestern Railroad between Council Bluffs and Crescent are especially fine.

The Aftonian is the water-bearing stratum, and everywhere springs and seepy places abound at its base.

3. At several points in this region Kansan drift rests unconformably upon the Aftonian. It is the typical bluish Kansan with an abundance of calcium carbonate in streaks, cloudings and concretions.

4. Perhaps more frequently, in the immediate vicinity of Omaha and Council Bluffs, the Aftonian is followed immediately by a deposit of joint clay which frequently shows stratification, and often contains sand and pebbles in its lower part. This is the deposit which the writer designated as the *Loveland*,[1] from the type exposure at Loveland, Iowa. Great deposits of Loveland, often exceeding 30 feet in thickness, occur on both sides of the river.

This formation, which probably belongs to the period of the melting of the Kansan ice, is of especial interest because it has usually been referred to loess, from which it differs in its joint clay texture, usually reddish color, absence of fossils, and frequent occurrence of pebbles and coarse sand-grains in its lower part.

5. Overlying the Loveland, and usually separated from it quite sharply, is a bed of characteristic post-Kansan bluish-gray loess, which is usually fossiliferous. This is displayed at several points near Florence and in South Omaha.

6. Upon the post-Kansan loess lies a bed of later yellow loess, which is also often fossiliferous.

The total thickness of the two loesses in this vicinity does not reach 35 feet at any point observed on the Nebraska side of the river, and its thickness on the Iowa side is much less than has been reported, since the thickness of the Loveland must be deducted.

[1] *Bulletin of the Geological Society of America*, Vol. 20, 1909.

The discovery of the great deposits of Nebraskan, Aftonian and Loveland is especially important. A more complete discussion of these deposits will soon appear.

B. SHIMEK

IOWA CITY, IOWA,
December 2, 1909

THE BOSTON MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

REPORT OF THE GENERAL SECRETARY

THE sixty-first meeting of the American Association for the Advancement of Science was held in Boston, during convocation week, 1909–10; the first general session was called to order in Huntington Hall at ten o'clock on the morning of Monday, December 27, 1909, by the retiring president, Professor T. C. Chamberlin, who introduced the president of the meeting, President David Starr Jordan. Addresses of welcome were made, on behalf of Massachusetts Institute of Technology by President Richard C. Maclaurin and on behalf of Harvard University by Dean Wallace C. Sabine. President Jordan replied briefly on behalf of the American Association. Announcements were made by the permanent secretary, the general secretary and the local secretary, after which the general session adjourned.

The various sections and the affiliated societies met in their respective halls, according to the published program, the Massachusetts Institute of Technology and Harvard University having placed their lecture halls and laboratories freely at the disposal of the association.

The address of the retiring president of the association, Professor T. C. Chamberlin, was given in Sanders Theater of Harvard University, on the evening of Monday, December 27, the subject being, "A Geologic Forecast of the Future Opportunities of our Race." This address was preceded by an address of welcome to Harvard University by Professor F. W. Putnam, and was followed by a reception given by the corporation of Harvard University to the members of the association and the affiliated societies and their accompanying ladies, in Memorial Hall.

The registered attendance of members of the association was 1,140, the largest in the history of the association. The registration by sections was as follows: A, 106; B, 124; C, 200; D, 36; E, 166; F, 218; G, 132; H, 92; I, 12; K, 50; L, 104. The registration of members of affiliated societies at the association headquarters was only 166. This conveys no meaning with regard to the attendance of affiliated members, as one instance will show; the registration at the headquarters of the American Chemical Society was 558, while only 200 registered as belonging to Section C of the association. No doubt the attendance of other affiliated members was large, and no registration was secured. It seems, therefore, that the attendance of scientific men may have exceeded 2,000.

GENERAL EVENTS

On Tuesday evening, December 28, a public lecture complimentary to the citizens of Boston, was given by Dr. C. W. Stiles, on "The Hookworm Problem in this Country in Reference to Public Health."

On Thursday evening, December 30, under the auspices of the Entomological Society of America, a lecture was given by Dr. John B. Smith on "Insects and Entomologists: Their Relation to the Community at Large."

A reception by the president and corporation of Massachusetts Institute of Technology to the members of the association and affiliated societies and their accompanying ladies was given on the afternoon of Wednesday, December 29.

A reception by President and Mrs. Maclaurin was given to the visiting physicists and their ladies, at their home on the afternoon of Thursday, December 30.

On the afternoon of Friday, December 31, a lecture was given by Dr. Percival Lowell on "The Canali Novæ of Mars."

The business meeting and banquet of the Society of the Sigma Xi were held on the afternoon and evening of Wednesday, December 29.

There were many dinners arranged for groups of members, such as mathematicians and astronomers, physicists, chemists, geologists, zoologists and entomologists, anatomists and physiologists; there were many less formal but very pleasant "smokers" and other gatherings at various hotels and club houses.

ITEMS OF GENERAL INTEREST FROM THE PROCEEDINGS OF THE COUNCIL

The council met at nine o'clock in the morning, on Monday, Tuesday, Wednesday, Thursday and Friday, December 27 to 31.

At these meetings 57 new members were elected. A much larger number had been elected quite recently, and should be considered as being elected at the Boston meeting. The membership is now more than 8,000.

The council elected 229 fellows from those proposed by the various sections.

The following were elected foreign associates for the Boston meeting: Dr. Hans Hallier, of Leyden; Mr. J. J. Taudin-Chabot, of Holland; Professor Franz Weidenreich, of Strassburg; Professor C. Runge, of Göttingen.

The council was authorized to elect to membership scientific men of Central and South America.

The resignation of Mr. R. S. Woodward as chairman of the committee on policy was accepted, and Professor E. L. Nichols was elected to fill the vacancy in the committee on policy.

The resignation of Mr. R. S. Woodward from the committee on organization and membership was accepted, and Dr. W. H. Welch was appointed to the committee as chairman.

It was decided that at this Boston meeting the program be given to members of the association only at the time of registration, and that persons not members be charged 25 cents for each copy.

The committee on the relation of plants to climate reported progress.

It was resolved that the inquiry regarding the cost of publication, distribution and use of publications of American scientific societies should be extended.

Grants of $75 each were made to the Concilium Bibliographicum Zoologicum, Dr. W. P. White, Professor G. J. Peirce and Professor T. D. A. Cockerell.

It was resolved that each recipient of a grant be asked for an itemized statement of expenditures.

It was resolved that the American Association for the Advancement of Science gives its approval to the general plan of the George Washington Memorial Association to collect funds for the purpose of erecting a building in the city of Washington adapted for a meeting place for national and other scientific societies, and other organizations, and that a committee of five be appointed to assist in the effort.

It was resolved, that with a view to the proper conduct of such investigations as will aid in lessening the waste of life and resources which now characterizes the mining industry of the United States, the American Association for the Advancement of Science respectfully urges the Congress of the United States to establish, during its present session, a national bureau of mines. Resolved, that copies of this resolution be sent to the Speaker of the House of Representatives, the President of the Senate and the President of the United States.

It was resolved that the American Association for the Advancement of Science approves the appointment of an eminent astronomer in charge of the Naval Observatory.

The permanent secretary was authorized to issue the volumes for the Baltimore and Boston meetings under one cover, with separate titles and indexes, but with only one list of members.

SECTIONAL MEETINGS

The meetings of the various sections, many in joint session with affiliated societies, were perhaps the most successful in the history of the association, as measured by the number of members attending, by the number and quality of the papers presented and by the interest with which the papers were heard and discussed. The number of papers presented before each section and its affiliated societies are approximately as follows: A, 37; B, 54; C, 254; D, 17; E, 141; F, 124; G, 122; H, 63; I, 17; K, 166; L, 30; total, 1,025.

It has been impossible to obtain a general report from every section, hence specific reports are not made here; the secretaries of the several sections will publish detailed reports.

GENERAL COMMITTEE

The general committee met at Hotel Brunswick, at 9:30 o'clock on the evening of Thursday, December 30. It was decided to hold the next meeting at Minneapolis, beginning, on the evening of Tuesday, December 27, 1910, with a general session of welcome, and the address of the retiring president. It was recommended that the meeting of 1911-12 be held in Washington, D. C.

The following officers were elected for the Minneapolis meeting:

President—Professor A. A. Michelson, University of Chicago.

Vice-Presidents and Chairmen of Sections:

Section A—Mathematics and Astronomy—Professor E. H. Moore, University of Chicago.

Section B—Physics—Dr. E. B. Rosa, Bureau of Standards, Washington, D. C.

Section C—Chemistry—Professor G. B. Frankforter, University of Minnesota.

Section D—Mechanical Science and Engineering—Professor A. L. Rotch, Blue Hill Meteorological Observatory.

Section E—Geology and Geography—Dr. John M. Clarke, state geologist of New York, Albany, N. Y.

Section F—Zoology—Professor Jacob Reighard, University of Michigan.

Section G—Botany—Professor R. A. Harper, University of Wisconsin.

Section H—Anthropology and Psychology—Professor Roland B. Dixon, Harvard University.

Section I—Social and Economic Science—The Hon. T. E. Burton, Cleveland, Ohio.

Section K—Physiology and Experimental Medicine—Professor F. G. Novy, University of Michigan.

Section L—Education—President A. Ross Hill, University of Missouri.

Permanent Secretary (for five years)—Dr. L. O. Howard, Washington, D. C.

General Secretary—Professor Frederic E. Clements, University of Minnesota.

Secretary of the Council—Professor John Zeleny, University of Minnesota.

Secretary of the Section of Social and Economic Science—Fred C. Croxton, Washington, D. C.

Treasurer—The selection of a treasurer, to succeed President R. S. Woodward, was referred to the council, with power.

CLOSING GENERAL SESSION

The closing general session was held in Huntington Hall at 10 o'clock on the morning of Friday, December 31, 1909; President Jordan presided.

An amendment to article 23 of the constitution, introduced by Mr. Gulliver at the Baltimore meeting, and approved by the council, was adopted. The amended portion of the article reads as follows:

Article 23. Immediately on the organization of a section, there shall be a member or fellow elected by ballot, after open nomination, who, with the vice-president and secretary and the preceding vice-president and secretary and the presidents and secretaries of those affiliated societies which shall be designated by the council and the members or fellows elected by ballot at the four preceding meetings, shall form its sectional committee.

Reports were read by the permanent secretary, the general secretary and the local secretary.

By resolution of the council the president expressed the thanks of the association for the great hospitality, courtesy and privileges extended to the members of the American Association, in connection with the Winnipeg meeting and the western excursion of the British Association for the Advancement of Science.

It was resolved that the association express its hearty thanks to the many institutions and individuals of Boston and Cambridge and vicinity who have contributed to make the meetings so successful and enjoyable. The following were mentioned:

The corporation of Massachusetts Institute of Technology.
The corporation of Harvard University.
The Museum of Fine Arts.
The Boston Society of Natural History.
Simmons College.
Boston University.
The local committee, Professor Charles S. Minot, honorary chairman; Professor H. W. Tyler, chairman of the executive committee.
Ginn and Company, donors of the Guide Books.
Boston Elevated Railway, for special cars.
The ladies' committee.
The citizens, for receptions, teas, musicales, dinners to restricted groups of members, too numerous for individual mention, but none the less sincerely thanked for the most generous hospitality shown throughout the meetings.

DAYTON C. MILLER,
General Secretary

SOCIETIES AND ACADEMIES

THE BOTANICAL SOCIETY OF WASHINGTON

THE fifty-eighth regular meeting of the society was held at the Dewey Hotel, December 18, 1909, at eight o'clock P.M., Vice-president W. J. Spillman presiding. The following papers were read:

Peridermium strobi, an Importation from Europe: Dr. PERLEY SPAULDING, U. S. Bureau of Plant Industry.

The European currant rust has two stages: one as a peridermium on the white pine, the other upon leaves of *Ribes*. The fungus is native in eastern Europe upon *Pinus cembra*, upon which it usually does little damage. Since about 1860 it has attacked *Pinus strobus*, *P. monticola* and *P. lambertiana*, all American species of pines. At present it is distributed throughout Europe, and is causing great damage to white pines in certain sections. In the spring of 1909 it was imported into the United States upon about two and one half million young white pine trees, being distributed in the states of New York, Vermont, New Hampshire, Massachusetts, Connecticut and Pennsylvania. Lots of trees from the same nursery are also known to have been imported into Ontario and Minnesota. During the past summer a special effort was made to remove the *Ribes*

from the vicinity of these plantations, and, it is believed, successfully, except in portions of Connecticut and in Ontario and Minnesota, which latter are inspected by local authorities. This work was carried on in cooperation with the forestry and plant pathological workers of the states involved. The disease is under control at present. The great problem now is to control or prevent further importations.

Chinese Perennial Wild Rice: C. S. SCOFIELD, U. S. Bureau of Plant Industry.

A plant closely resembling the wild rice of North America was collected about seventy years ago in the Trans-Baikal region of Siberia by a Russian botanist, Turczaninow. At that time the American wild rice was known to European botanists under the name *Hydropyrum esculentum*, and Turczaninow's plant, being regarded as congeneric with it, was named *Hydropyrum latifolium*. When Hackel monographed the grasses for Engler and Prantl, he decided that the Asiatic plant was only a variety of the American species, and, resurrecting the Linnæan name, *Zizania aquatica*, applied it to both.

Certain significant characters indicate that the Asiatic plant is a distinct species from the American. The American plant is an annual, being reproduced by seed which falls off into the water as soon as ripe. The Asiatic plant is perennial, capable of reproduction by rhizomes. There are also some differences in the floral characters, these being most apparent in the form of the floral pedicel and in the length of the awns of the glumes.

The Close Parallel between the Floras of Palestine and of California: Professor A. AARONSOHN, Haifa, Palestine. (By invitation.)

The speaker first exhibited a series of lantern slides showing the topography of Palestine, and the striking resemblances to that of California. Near the coast in each region is a range of low mountains beyond which lies a long interior valley having a range of higher mountains for its eastern wall. The direction of the prevailing winds being the same, the distribution of rainfall is closely parallel.

Herbarium sheets were then displayed showing numerous specimens of the same species from each region where they occur under very similar climatic and topographic conditions.

The conclusion was drawn that the present unfavorable aspect of agriculture in Palestine is not due so much to sterility of soil and aridity of climate as to the adverse influence of the form of government which has prevailed there. Under an improved régime it is anticipated that many of the economic plants that now flourish in California may be successfully introduced into Palestine.

In conclusion Professor Aaronsohn presented a second series of lantern slides showing agricultural conditions in Palestine to-day, and the vegetation of the sections visited in his explorations which resulted in the discovery of a wild wheat growing at high altitudes on sterile soil which he considers to be the prototype of our modern cultivated varieties. He found in this wild species a great diversity of types, some forms resembling *Triticum durum* and others *T. polonicum* and *T. monococcum*.

W. W. STOCKBERGER,
Corresponding Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 39th annual meeting of the society was held in the West Hall of George Washington University on December 18, 1909, President Wead in the chair. The meeting was devoted to the presentation of the usual annual reports and the election of officers.

The following officers were duly elected for the ensuing year:

President—R. S. Woodward.

Vice-presidents—C. G. Abbot, A. L. Day, L. A. Fischer, E. B. Rosa.

Treasurer—L. J. Briggs.

Secretaries—R. L. Faris, W. J. Humphreys.

General Committee—W. A. DeCaindry, Edgar Buckingham, P. G. Nutting, E. G. Fischer, R. A. Harris, W. S. Eichelberger, F. A. Wolff, G. K. Burgess, B. R. Green.

R. L. FARIS,
Secretary

THE WASHINGTON CHEMICAL SOCIETY

THE 194th meeting of the society was held at the George Washington University on Thursday evening, December 9, 1909. President Walker presided, the attendance being 55. Dr. Eugene T. Allen resigned as councilor of the American Chemical Society, and W. B. D. Penniman was elected in his stead. The following papers were read: "Estimation of Glycerine in Meat Preparations," by F. C. Cook; "Enzymes and their Relation to Soil Fertility," by M. X. Sullivan; "Detection of Colocynth Seed in Powdered Colocynth," by . K. Chestnut.

J. A. LE CLERC,
Secretary

THE AMERICAN CHEMICAL SOCIETY
NEW YORK SECTION

THE third regular meeting of the session of 1909–10 was held at the Chemists' Club on December 17.

The following papers were presented: Morris Loeb and L. R. Morey, "Analyses of some Antique Bronzes"; Chas. Baskerville, "The Action of Radium Salts upon Ruby"; Chas. Baskerville and Reston Stevenson, "Apparatus for Drying Flasks."

Notice was given that members are invited to transmit to the secretary the titles of papers descriptive of new apparatus for presentation at the March meeting, which has been set apart for a symposium on new apparatus and lecture experiments.

C. M. JOYCE,
Secretary

THE ASSOCIATION OF TEACHERS OF MATHEMATICS IN THE MIDDLE STATES AND MARYLAND

The thirteenth meeting of the association was held at the College of the City of New York on Saturday, December 4.

The day was given up to the reading of two papers, "Mathematics in the Ethical Culture High School," by Charles B. Walsh, of New York City, and "Some Suggestions in the Teaching of Geometry," by Isaac J. Schwatt, of the University of Pensylvania, and to the reports of the various committees. The most important reports were the report on the association publication, *The Mathematics Teacher*, by the editor, William H. Metzler, of Syracuse University, and the report of the committee on algebra syllabus, presented by the chairman, Gustave Legras, College of the City of New York.

Dr. Metzler's report showed that *The Mathematics Teacher* was unqualifiedly a success; letters of commendation are frequently received, the subscription list outside of the association is increasing, and the financial side is in surprisingly good condition.

The algebra syllabus was discussed in detail, and after some amendments, was adopted by the association. The committee was continued, with power to make any necessary alterations in the preliminary report, and to make out the syllabus for advanced algebra.

The association passed an amendment to the constitution providing for the election of the editors of *The Mathematics Teacher*, and then elected the following officers for the following year:

President—William H. Metzler, Syracuse University.

Vice-president—Daniel D. Feldman, Erasmus Hall High School, Brooklyn.

Secretary—Eugene R. Smith, Polytechnic Preparatory School, Brooklyn.

Treasurer—M. Edna Shaw, William Penn High School, Philadelphia.

Members of the Council—(two years) Paul N. Peck, George Washington University, Washington, D. C.; (three years) Howard F. Hart, High School, Montclair, N. J.; Isaac J. Schwatt, University of Pennsylvania.

Editor-in-chief—William H. Metzler.

Associate Editors—Eugene R. Smith; Jonathan T. Rorer, William Penn High School, Philadelphia.

The secretary was instructed to report for the association at the federation meeting in Boston, December 27, and the editors, with the retiring president and the newly elected treasurer, were appointed a committee to confer with other associations, and especially with the federation, on the question of official publications.

EUGENE R. SMITH,
Secretary

POLYTECHNIC PREPARATORY SCHOOL,
BROOKLYN

THE NEW YORK SECTION OF THE ASSOCIATION OF TEACHERS OF MATHEMATICS IN THE MIDDLE STATES AND MARYLAND

THE first meeting for the year 1909–10 of the New York Section of the Association of Teachers of Mathematics in the Middle States and Maryland was held Friday evening, November 12, at the High School of Commerce, New York City. The topic for the evening was "Mathematics for Service or for Culture." "Mathematics for Service" was presented by Dr. Ernest R. van Nardroff, principal of the Stuyvesant High School, New York City. Dr. van Nardroff spoke from the experience of an able physicist, and offered a course of study, including the topics of algebra, geometry, trigonometry, analytical geometry and the calculus, which are useful in physics. "Mathematics for Culture" was presented by Dr. William H. Metzler, professor of mathematics, Syracuse University, Syracuse, N. Y. That Dr. Metzler spoke convincingly, or that the opinion of the meeting was already formed was evidenced by the discussion that followed. This discussion was animated and largely in favor of pure mathematics for its own sake.

LAO G. SIMONS,
Secretary